



PHD

From Birmingham to Berlin: skill-transfer, replication and embedded technical knowledge in the development of microwave radar in Britain and Germany, 1939-45

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From Birmingham to *Berlin*:

**Skill-Transfer, Replication and Embedded Technical Knowledge in the
Development of Microwave Radar in Britain and Germany, 1939-45.**

Submitted by Stephen Travis

for the degree of PhD
of the University of Bath
August 1995

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**Dedicated to my Mother and Father, without whom neither I nor
this would have been possible. Thank you for your love and
support over the years.**

Abstract

This thesis is concerned with the role of skill and tacit knowledge in the form of embodied knowledge in researchers, and embedded “black-boxed” knowledge in the form of artefacts, in the development of innovative scientific and technical practices and apparatus. What forms does the embodied skill and tacit knowledge of researchers take? Is this sort of knowledge required for there to be a transfer of scientific knowledge? Can embedded scientific knowledge be uncovered by researchers lacking the acquired tacit knowledge and skills to build and/or operate new scientific apparatus if they are in possession of that apparatus?

I provide answers to these questions by considering the case of the development of microwave radar in Britain, and its later development through the discovery of a British example by the Germans. Longer (metre) wave radar was developed simultaneously, though independently, in Britain and in Germany during the 1930s. During this pre-war period researchers in each country learned basic skills about how to design and build working radars at metric wavelengths. On the outbreak of war, strategic pressures forced the British to seek to develop radar using the hitherto barely explored microwave (centimetric) region of the electromagnetic spectrum. This region was ignored in Germany through a combination of a lack of strategic reason to do so, and beliefs that microwaves would not prove useful for radar anyway. I have gathered information on this topic by using secondary sources such as books and articles, primary sources in archive collections, and interviews of some participants.

The importance of tacit knowledge was argued by Ryle in the 1940s and Polanyi in the 1950s, but only in the 1970s did it come to be investigated more widely. The work done in that period by Collins has been added to by Gooding and now several others. I use this work as the basis of my examination of these topics in conjunction with the British development of microwave radar between 1940 and 1942, and the German copying of this equipment and research programme instigated when they recovered an example from an aircraft crash in 1943.

Acknowledgements

This thesis was completed with the assistance of a quota studentship from the (now defunct) SERC. I was able to use their provisions for travel in order to visit archives in the UK and Germany. I would also like to acknowledge the financial support and understanding of my parents, and the National Westminster Bank, in keeping me “afloat” whilst this work was undertaken.

I would like to thank the people who introduced me to the field of History, Sociology and Philosophy of Science, and to the subject of radar; this thesis is a marriage of the two. The HPS Department at Cambridge University aroused my interest in this subject, in particular the lectures of Dr Simon Schaffer. Thanks also to Jeff Hughes and Alex Bird for interesting, informal and above all useful supervisions. Finally, thanks to the Royal Air Force for conscripting my father, whose tales of life as a radar fitter introduced me to the topic of radar and to the cavity magnetron, and gave me the idea for the thesis.

I owe a large debt to my academic supervisor in the School of Social Sciences, Dr David Gooding, for helping me to organise my rather vague ideas into something approaching order, and for extremely useful discussions on how to analyse and present my work. I also wish to thank Professors Collins and Buchanan for their assistance and comments.

Several people who appear in this thesis as actors have given me invaluable assistance. Professor Sir Bernard Lovell has allowed me access to all his personal material relating to H₂S and his wartime work, and permitted me to question him on it, for which I am very grateful, as I am too for arranging access to the archives at Manchester University. Reginald Batt very kindly answered some questions on his role in events, and drove me around the sites of interest in and around Worth Matravers in Dorset. Professor William Burcham at Birmingham University permitted me to read his laboratory notes, and talked to me about his wartime work, as did Sir Robert Clayton of GEC. Thank you also to E.B. Callick, Sir Alan Hodgkin, Professor R.V. Jones and the late Group Captain Dudley Saward for their correspondence.

I have used material from many different sources, and received much assistance in order to do so. I appreciate greatly the assistance of the staff of the Public Records

Office at Kew, the Royal Air Force Museum in Hendon, the Imperial War Museum in Lambeth, the Bundesarchiv in Koblenz and the Militärarchiv in Freiburg for enabling me to locate the original material quoted in the text. I am very grateful to Professor Sean Swords of Trinity College, Dublin, for sending me a complete set of minutes of the *Arbeitsgemeinschaft Rotterdam*, something I could get hold of by no other means. I would also like to thank Dr Braun of the Militäruniversität Hamburg for providing me with information on German material, and Dr John Bryant for some American-based material.

My colleagues in 3E 4.27 have had to put up with me for more than their fair share of time. Their patience has been great, and their help and support very gratefully received. Many thanks go to Gill, Jo and Judith for answering my occasional queries, and providing an eventful background to life in the office. To Warren and Rob, thanks for your assistance, and for providing competition in various ways. Thanks also to all the staff in the School of Social Sciences for assistance with various matters.

Finally I would like to thank several people who have helped me on a personal level over the years I have been working on this thesis. Thanks to all my various friends, but especially to Mark for moral support and for being an impeccable housemate, to Charlie for crashing on my floor, and to Hilary for food and conversation. Thanks to the Cookes for similar hospitality, especially when I fractured my arm. Thank you to Vivien, for two eventful years of support. Thanks to Helen, for your ability to beat me at Scrabble, and for your exacting editorial work. Thea, thank you for being “my sister”, especially post-Durham, and my parents for everything and to whom this work is dedicated. Lastly, thank you to all the members of the Bath University 1994 Thames Cup VIII at Henley Royal Regatta, for allowing me to put my work into perspective.

If anyone else not mentioned personally feels they are owed recognition for their assistance in any way either to me or this thesis, then here is where I thank you. It has been a very enjoyable and satisfying four years, and I hope this work will be a tribute to you all.

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Glossary

AI	Airborne Interception radar: a type of airborne radar used to locate other aircraft.
AGR	<i>Arbeitsgemeinschaft Rotterdam</i> : German committee set up to investigate the captured H ₂ S, codenamed <i>Rotterdam</i> .
AMRE	Air Ministry Research Establishment: the designation of the radar research establishment during 1939-40.
ASV	Anti Surface-Vessel radar: a type of airborne radar used to locate ships and submarines.
<i>Berlin</i>	German redesigned copy of H ₂ S.
Cavity Magnetron	Type of thermionic valve producing radiowaves on centimetric wavelengths at high powers. A central cathode is surrounded by an anode block which has cavities arranged around the axis (see chapter 3).
CH	Chain Home: coastal early-warning radar.
CHL	Chain Home Low: a rotating radar introduced to cover the gap where CH couldn't "see".
CRT	Cathode Ray Tube: type of display valve (or tube) where electrons are fired at a screen in a vacuum tube.
CSSAD	Committee for the Scientific Survey of Air Defence
CSSAO	Committee for the Scientific Survey of Air Offence.
CVD	Co-ordination of Valve Development committee.
DCD	Director of Communications Development: a Civil Service post in the Air Ministry.
Doorknob	Eponymously shaped thermionic valve used in 1.5m AI.
EMI	Electrical Musical Industries: an electric and electronic corporation.
GEC	General Electric Company.
H ₂ S	A British ground-mapping centimetric radar.

Klystron	A valve where a beam of electrons pass through two dough-nut shaped resonant cavities to produce centimetric waves (see chapter 3).
<i>Lichtenstein</i>	German AI radar.
MAP	Ministry of Aircraft Production.
RAE	Royal Aircraft Establishment, Farnborough.
RAF	Royal Air Force.
<i>RFR</i>	<i>Reichsforschungsrat</i> : German Research Bureau.
<i>Rotterdam</i>	German rebuild of H ₂ S.
Split-anode magnetron	A glass-envelope thermionic valve where the central cathode is surrounded by a cylindrical anode, which is divided into two or more segments. A magnetic field is applied along the axis of the anode/cathode combination.
<i>Telefunken</i>	Main German electronics company, concerned with building <i>Rotterdam</i> and <i>Berlin</i> .
T/R	Transmit/Receive: unit, or method, to switch a single aerial between these two features.
TRE	Telecommunications Research Establishment: the designation of AMRE after 1940.
Window/ <i>Düppel</i>	Small strips of metal foil dropped by aircraft to confuse enemy radars. The clouds of the strips act as tuned reflectors.

Selected Biography

Airey, J.E.	Bowen's technician at Orfordness, 1935.
Appleton, Sir Edward	Ionospheric Research pioneer, during the 1920s, and Cavendish Physicist.
Atkinson, James	Cavendish physicist who joined Bawdsey in 1939, and worked on the AMRE centimetre team in 1940.
Bainbridge-Bell, L.H.	Ionospheric worker in the 1930s, who worked on CH in 1935-6.
Batt, Reginald	Former Post Office engineer who became a technician on the centimetre team at AMRE in 1940.
Bennett, Don	Head of No. 8 Group, the Pathfinder Force at Bomber Command, during the war.
Blackett, P.M.S.	Former Cavendish physicist who was head of Manchester University Physics Department before the war. He was a member of the CSSAD.
Blumlein, Alan	EMI electronic engineer who designed the first usable television before the war. Worked on radar during the war, and was killed in an aircraft whilst working on H ₂ S in June 1942.
Boot, H.A.H.	Research physicist at Birmingham University who invented the cavity magnetron with Randall in February 1940.
Bowen, Dr E.G.	Originally employed in Watson-Watt's Radio Research Laboratory at Slough, in 1933. Moved to Orfordness in 1935, and Bawdsey in 1936. Appointed head of British airborne team in 1936. Went to USA with Tizard Mission in 1940.
Bragg, Sir Lawrence	Head of the Cavendish Laboratory during the 1930s and 1940s.
Brandt, Dr	German <i>Telefunken</i> engineer in charge of the centimetre research programme after the discover of <i>Rotterdam</i> .
Burcham, Prof. W.E.	Cavendish physicist, under Dee, conscripted into AMRE at the outbreak of war. Worked on centimetre AI 1940-5.
Butt, D.M.	Member of War Cabinet Secretariat who undertook the survey into bombing in July 1941.

Chapman, A.H.	Lovell's technician from Manchester University, who assisted with Lovell's experiments during 1939-40
Cherwell, Lord	See Lindemann, F.
Clayton, Sir Robert	Member of GEC's television team pre-war, who worked on 25cm AI during 1939-40.
Coales J.F.	Valve designer and engineer, head of Admiralty Signal School valve-design group in the late 1930s and during the war.
Cockroft, Sir John	Cavendish physicist who worked on applications of centimetre radar. Originator of Operational Research.
Cooke-Yarborough, E.H.	AMRE engineer given the job of checking the minimum range of AI problem.
Dee, Prof. P.I.	Cavendish physicist conscripted into AMRE at the outbreak of war. Headed the centimetre team 1940-5.
Dickie, John	Assistant to Saward at the Bomber Development Unit, who helped with the pre-production design on H ₂ S.
Dippy, R.H.	Inventor of GEE system.
Dowding, Marshall of the RAF Sir Hugh	Air Member for Research and Development in 1935, who voted money for the first CH radar. Better known as head of Fighter Command during the Battle of Britain in 1940.
Epsley, Dr D.C.	Head of GEC's television team, who worked on 25cm AI during 1939-40.
Esau, Prof.	German physicist in charge of centimetre research at <i>Telefunken</i> in the late 1930s, early 1940s, before Brandt.
Göring, Hermann	Head of the <i>Luftwaffe</i> , and number 2 in the Nazi state after Hitler.
Gutton, Dr Henri	French engineer with SFR, and inventor of the oxide-coated cathode for split-anode magnetrons.
Hensby, G.S.	Member of the centimetre AI team at AMRE/TRE, who subsequently worked on H ₂ S.
Hanbury-Brown, Sir Robert	Physicist who was appointed to Bowen's airborne radar team in 1936. He went on to work with metre-wave AI during 1940-1.

Hansen, William	American physicist who worked with the Varian brothers to design the klystron, at Stanford University between 1936-40.
Harris, Sir Arthur	Commander in Chief of Bomber Command between March 1942 and 1945. Architect of the Area Bombing policy.
Hill, A.V.	Physiologist at University College, London, and member of the CSSAD.
Hodgkin, Sir Alan	Cambridge physiologist conscripted into AMRE during the war. Went to work with Lovell on centimetre research during February 1940. Designed the spiral-scanner for centimetre AI during 1940.
Hull, A.W.	American inventor of the conventional glass-envelope magnetron.
Hülsmeier, Christian	Inventor of the “first” radar, a ship-mounted obstacle-detector, patented in 1904.
Ingleby, Peter	Manchester University physicist, conscripted into AMRE at the outbreak of war, and who joined Lovell’s team at St Athan. Killed in an air crash in January 1940.
Jones, R.V.	Physicist under Lindemann at the Clarendon Laboratory during the 1930s, who worked in infra-red detection of aircraft. At the outbreak of war he was conscripted into government service, and worked in scientific intelligence for the rest of the war.
Joubert, Sir Philip	Head of Fighter Command 1940-5. Succeeded Dowding.
Killip, Len	Member of the Bomber Development Unit, who devised the method of navigation using H ₂ S.
Kühnhold, Dr Rudolf	German physicist, and head of GEMA company. First person to become interested in radar research in Germany.
Larnder, H.	Head of the RAF’s Operational Research Section.
Lee, Sir George	Director of Communications Development during the war.
Lewis, W.B.	Cavendish physicist appointed in July 1939 by Rowe as deputy superintendent of AMRE.
Lindemann, F.	Head of the Clarendon Laboratory, Oxford, during the 1930s. Became Churchill’s Scientific Advisor during this period, a position made official on Churchill’s appointment

as Prime Minister in May 1940. Appointed Lord Cherwell in 1942.

Lovell, Sir Bernard	Gained PhD in Physics at Bristol University under Skinner in 1936. Moved to Manchester University under Blackett, until conscription into AMRE in 1939. Worked on centimetre AI 1940-1, then made head of H ₂ S 1942-5.
Martini, General	Head of the <i>Luftwaffe</i> signals Unit, and in charge of the introduction of radar before and during the first part of the war.
Megaw, E.C.S.	Engineer and valve-designer with GEC. Worked on split-anode magnetrons during the 1930s. Was responsible for designing the pre-production cavity magnetron in 1940.
O’Kane, Dr B.J.	Member of GEC’s television team, who worked on 25cm AI during 1939-40 and went on to work with Lovell on H ₂ S during 1942-4.
Oliphant, Sir Mark	Cavendish physicist from Australia. Moved to become head of Birmingham University Physics Department in 1938.
Paterson, Sir Clifford	Wartime head of GEC Research Laboratories at Wembley.
Posthumus, K.	Dutch engineer, employed by Phillips, and inventor of the split-anode principle for the glass-envelope magnetron in 1934.
Quilter, Sir Cuthbert	Original owner of Bawdsey Manor, removed in 1936 when the radar establishment relocated there.
Rabi, I.I.	American head of the Radiation Laboratory at MIT.
Randall, J.T.	Former GEC engineer and physicist who joined Oliphant’s team at Birmingham University. Invented the cavity magnetron with Boot in February 1940.
Reeves, R.H.	Inventor of OBOE system.
Renwick, Sir Robert	In charge of the production of four-engined bombers and radio navigation aids at the Ministry of Aircraft Production during the war.
Robinson, D.M.	Engineer who joined AMRE in 1939. Worked on centimetre radar during 1940. Went to USA in 1941 to liaise with the newly formed MIT Radiation Laboratory.

Rowe, A.P.	Civil Servant made Secretary to the CSSAD in 1934. Appointed head of Bawdsey in succession to Watson-Watt in 1938.
Rukop, Dr	Head of the <i>Telefunken</i> research & development department.
Runge, Dr Wilhelm	Head of receiver research at the <i>Telefunken</i> laboratory. Worked on <i>Rotterdam</i> and <i>Berlin</i> .
Saward, Dudley	Officer in charge of the introduction of radar to Bomber Command during the war.
Saundby, Sir Robert	Deputy Commander in Chief of Bomber Command, to Harris.
Sayers, James	Former Cavendish and ionospheric research physicist who joined Oliphant at Birmingham University. Invented the strapping principle for cavity magnetrons.
Schoenberg, Sir Isaac	Head of EMI during the war.
Schultes, Dr	German physicist, and Kühnhold's deputy at GEMA.
Skinner, H.W.B.	Former Cavendish physicist who was head of Bristol University Physics Laboratory during the 1930s. Conscripted into AMRE at the outbreak of war, he commenced centimetre research in March 1940 and eventually became head of centimetre research later that year.
Smith, Sir Frank	Head of the Department of Scientific and Industrial Research during the late 1930s and during the war.
Southworth, G.C.	American researcher who worked on waveguides during the 1930s at MIT.
Speer, Albert	Hitler's Chief Architect in the late 1930s and the first half of the war. He was appointed to run the economy following the death of Todt in early 1942, and significantly increased production during the period of greatest disruption caused by bombing.
Steimel, Dr	German physicist working on centimetre research at <i>Telefunken</i> after the discovery of <i>Rotterdam</i> .
Sutton, R.W.	Member of the Admiralty Signal School, and designer of the "Soft Sutton" reflex klystron in 1941.

Swinton, Lord.	Head of the Air Ministry, and head of the sub-committee of the Committee for Imperial Defence, set up in 1935 which included Lindemann
Tizard, Sir Henry	Rector of Imperial College in 1934, when asked to form a committee to review British Air Defence. Continued to serve on this committee until 1942, when sidelined due to the promotion of Lindemann.
Todt, Dr	A German civil engineer who came to prominence by designing the Autobahn network in the 1930s. He headed the Todt Organisation in the Nazi state, which ran all construction projects. His responsibilities were taken over by Speer after his death.
Touch, Gerald	Clarendon physicist who joined Bowen's airborne radar team in 1937, and who went on to work at AMRE in 1939-41.
Udet, General	Head of the Luftwaffe procurements department, who was a former WW1 fighter pilot.
Varian, Russell	Stanford physicist who worked with his brother and Hansen on the klystron, 1936-40.
Varian, Sigurd	American Pan-Am pilot who worked with his brother and Hansen on the klystron, 1936-40.
Ward, A.G.	Canadian physicist who worked on centimetre receivers at AMRE in 1940.
Watson-Watt, Sir Robert	Self-styled "Father of Radar." An ionospheric worker in the early 1930s, he was consulted about the possibility of using radio waves to locate and destroy aircraft by members of the CSSAD. After using Wilkins to do the calculations, he initiated research on radar and was made head of Bawdsey. In 1938 he was appointed to the Air Ministry as Director of Communications Development.
Wilkins, A.F.	Ionospheric researcher, who calculated the original radar equation in 1935, and who later worked on CH at Bawdsey.
Wimperis, H.E.	Appointed as Director of Scientific Research, Air Ministry, in 1924. Set up CSSAD in 1934
Wright, C.S.	Admiralty Director of Scientific Research, and head of the CVD Committee when it was formed in 1939.
Yagi, Prof.	Japanese inventor of the Yagi Array radio antenna.

Chronology of Radar Development

Date	Britain	Germany
May 1934		Kühnhold begins radar research. GEMA founded.
26/2/35	Daventry Experiment	
June 1935	First transmitter working at Orfordness.	GEMA test their system at Friederichshaven.
March 1936	Research group move to Bawdsey. Bowen forms airborne group.	
Late 1938	Bowen's group test ASV.	Freya radar enters service with Navy.
June 1939	Fitting of AI Mark I begins.	German flights over Channel to monitor CH transmission.
Sept. 1939	War breaks out. Bawdsey moved to Dundee.	War breaks out.
Late 1939	Move of airborne group to St Athan.	Experiments with models indicate centimetres unsuitable for aircraft detection.
21/2/40	Randall & Boot operate cavity magnetron.	
Early 1940	Commencement of centimetre experiments at Dundee and St Athan.	Enquiry by High Command over the possibility of airborne radar.
May 1940	AMRE moves to Worth Matravers.	
Summer 1940	Minimum range problem with Marks I-III 1.5m AI.	Martini approaches Runge about building German AI.
August 1940	First 10cm echoes. Demonstrations to GEC and members of staff.	
Autumn 1940	Introduction of Mark IV AI.	Trials of first <i>Lichtenstein</i> AI.
February 1941		Relaxation of rule disallowing external protrusions on aircraft.

Date	Britain	Germany
March 1941	First airborne tests of 10cm AI.	German night-fighter pilot disallowed from using <i>Lichtenstein</i> prototype after he is too successful.
May 1941	Mark IV AI is used increasingly successfully against German bombing raids.	—
June 1941		Germany invades Russia
Jul-Sep 1941	Investigation into failure of British night-bombing.	
26/10/41	“Sunday Soviet” meeting about bombing aids.	
Nov 1941	Tests using prototype 10cm AI to investigate possibility of town-finding.	
23/12/41	Secretary of State for Air’s meeting orders commencement of research into H ₂ S.	
Jan 1942	Lovell appointed head of H ₂ S project.	
Feb 1942	Bruneval raid captures intact Würzburg.	Speer appointed head of German war economy. <i>Lichtenstein BC</i> AI introduced into service.
16/4/42	First flight trials of H ₂ S.	
7/6/42	Crash of Halifax V977, killing several members of H ₂ S team.	
3/7/42	Churchill demands two squadrons of H ₂ S-equipped aircraft by autumn.	
15/7/42	Restrictions on flying cavity magnetron over Germany lifted.	
Oct 1942	Flight trials of H ₂ S at Bomber Development Unit. Introduction into service of first Mark VIII 10cm AI.	

Date	Britain	Germany
Nov 1942	Production and equipping of first PFF squadrons with H ₂ S.	Disarray in German electronics industry. Centimetre research again rejected as unsuitable for radar.
31/1/43	First operational use of H ₂ S.	
3/2/43	Second operational use of H ₂ S.	Capture of H ₂ S, named <i>Rotterdam</i> .
March 1943		Discovery of purpose of H ₂ S after interrogation of operator. First H ₂ S severely damaged in raid on <i>Telefunken</i> works.
May 1943	Beginning of mass-production of H ₂ S for main bomber force.	Capture of second, slightly damaged H ₂ S to replace severely damaged first set.
June 1943		Preparation of flight-testing for rebuilt H ₂ S. Commencement of <i>Naxos</i> cm-warning receiver programme. Decision to build German-designed <i>Berlin</i> .
Summer 1943	Use of H ₂ S on Hamburg raid.	Capture of Mark VIII 10cm AI.
Sept 1943		Installation of <i>Rotterdam</i> into an aircraft for flight-trials.
8/2/44		Brandt lecture on state of centimetre research in Germany.
Spring 1944	Heavy losses to bomber force.	Commencement of <i>Rotterdam</i> and <i>Berlin</i> flight-trials.
6/6/44	Invasion of France	Invasion of France.
Autumn 1944	Heavy bombing begins to severely disrupt German industry. H ₂ S cleared of causing heavy losses.	
March 1945		Flight-trials of prototype centimetre <i>Berlin N1</i> AI.
8/5/45	Surrender of Germany.	Surrender of Germany.
June 1945	Capture of German equipment and interrogation of radar personnel.	

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Bibliographical Notes

Bibliographical Form

Books are listed in the following manner:

Author, A.N. (date) *Title*, Place: Publisher.

Journal Articles are listed in the following manner:

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Archival Collections

The reference numbers footnoted in the text refer to papers held in the following archive collections:

Public Records Office, Kew	AIR14, AIR20, AVIA10, AVIA15, AVIA26, AVIA36, AVIA39.
Bundesarchiv, Koblenz	R26III.
Militärarchiv, Freiburg	RL36, RL39.
RAF Museum, Hendon	AP1093C, AP1093D, AP1136, AP2890L, CD0419A, CD0419B.

Journal Abbreviations

The following abbreviations for journals are employed:

Biog Mem. R. Soc.	Biographical Memoirs of the Fellows of the Royal Society.
HMSO	Her Majesty's Stationary Office.
I.E.E. Proc.	Institute of Electrical Engineers Proceedings.
I.E.E.E.	Institute of Electrical and Electronic Engineers.
J.A.I.E.E.	Journal of the American Institute of Electrical Engineers.
J.I.E.E.	Journal of the Institute of Electrical Engineers.
Phys. Review	Physical Review.

Chapter 1: Introduction

It is a truism amongst real-ale drinkers that their favourite tippie does not "travel". It can only be enjoyed at its best near to its place of origin. Real-ale brewers often assert that the particular flavour and colour of their produce is the result of processes that cannot be explained "scientifically". Their beer is the product of the brewers art: it comes from the skill of its maker. This thesis investigates whether technical knowledge in both its physical and embodied forms is like beer. How is it made, and how well does it travel?

1.1 Topic of Investigation

This thesis is a historical investigation into the development of microwave radar in Britain, and its subsequent copying in Germany. These events took place during the Second World War, with British microwave radar development taking place between October 1939 and February 1943, and the German development taking place between 1943 and their capitulation in May 1945. I will examine the historical events within a theoretical framework that I will outline in the next section.

Radar¹, a method of object detection using radio-waves, was developed simultaneously in several countries during the 1930's. The first systems relied largely on the proven components and techniques of the radio industry, of atmospheric research, and of the fledgling television industry. Descriptions of these components and techniques were widely published in the scientific press during the later 1930's. However, there was no publication

¹ The American acronym 'radar' (from RAdio Detection And Ranging) only came into widespread use in Allied countries in 1944. Prior to this the British used the acronym RDF (from Radio Direction Finding), and the Germans referred to *Funkmeßapparat*. For clarity's sake I will refer throughout this thesis to all apparatus as being radar, although this usage is strictly anachronistic in the same way that referring to pre-C19th Natural Philosophy as Science is anachronistic. Swords (1986), chapter 2.

of the radar application of these techniques. As a result, until the outbreak of war scientists and engineers in Britain and Germany were unaware of the advances made by in this field by their colleagues in the other country².

During 1939/40 the British became interested in the possibility of using much shorter radio waves as a means of producing airborne radars with a longer range and greater accuracy than those using the hitherto conventional technology. Such a development was encouraged, as senior scientists saw it as the solution to the problem of countering night-bombing, something greatly troubling to them and to senior British politicians and military leaders. They realised that their newly-developed Coastal Defence Chain radars would severely restrict daylight operations by the enemy, who would then be forced to bomb at night when the radar chain was useless; British fighters being unable to operate in darkness.

Until this time very short waves were largely ignored for radar purposes, as the means of producing them at sufficiently high power did not exist. Similar experiments using them for communications links in the mid 1930's also ceased for the same reason. The propagation and transmission characteristics of these microwaves were not studied in any depth in Britain. Greater interest was shown by researchers in the United States, which gave the British a small body of knowledge in the field on which they could draw. During 1940-41 British researchers undertook intense activity in this area, and produced suitable components for microwave radar including the revolutionary cavity magnetron valve. This proved to be the key to high power microwave generation, which then allowed experimentation into airborne microwave radars for locating other aircraft (centimetre AI).

In 1942 British researchers carried out experiments to adapt their prototype microwave airborne search radar into a terrain-mapping system to be used in bombers. The pressure for this system came from a change in the political and military strategy of the Allies, who now favoured putting increasing resources into Strategic Bombing. However, an analysis of

² This also applied to every country who developed radar at around this time, among whom were France, the USA, Japan, the Netherlands, and Hungary. Burns (1988, ed.).

bombing accuracy undertaken during Spring 1941 showed that the majority of bombs dropped by the RAF fell nowhere near their intended target. Before wholesale strategic bombing could be justified in military terms, a solution to this problem had to be found. One of the various solutions proposed was using centimetric radar for terrain-mapping and blind-bombing. The system developed from these ideas was known as H₂S, and was first flown on operations in February 1943.

The Germans had manufactured well-engineered radar sets using the conventional technology well before 1939. Due to a variety of reasons research into microwave radar was abandoned by them in 1941. On the second operational mission that H₂S was used by the British, an aircraft bearing an example of the apparatus was shot down over Rotterdam in Holland. The new equipment was examined by the Germans, and with the help of two or three further recovered examples, rebuilt over the following year.

Once the equipment had been examined, a programme of research was initiated in Germany to fully understand the technical advances made by the British (and the Americans, who had received the cavity magnetron valve in 1940, and had begun a microwave research programme themselves). This programme led to the adaptation of microwave techniques to the Germans' own uses. The first thing they did was to put a detector into their night-fighter aircraft which they used to home on to transmissions from British H₂S sets, enabling their night-fighters to track the British bomber 'streams'. The Germans, having first rebuilt H₂S as a ground-mapping system named *Berlin* system, confined their later experimentation into turning it into an AI system. They went full circle on the British (who turned their centimetre AI into a ground-mapping system for bombers) due to a change in strategic priorities that was forced on them by their worsening war situation. No German microwave radar equipment was advanced beyond the research stage before hostilities ceased in 1945. Some of their apparatus, and some of the scientists who had designed and developed them, were captured and interviewed by allied commissions immediately following the capitulation.

Their testimonies, coupled with Allied assessments of the captured equipment, form an important resource about the German side of events.

1.2 Theoretical Basis

As I have outlined very briefly in the previous section, this thesis is a comparative history of the development of microwave (centimetric) radar in Britain and Germany during the Second World War. What I also wish to do, apart from relate these historical events, is to set them in a theoretical framework that will allow me to make an analysis of the material I have gathered within the context of historical debate. This framework takes the form of three major themes, and one minor theme. These themes have emerged out of issues that are important within the field of Science Studies.

I use the term Science Studies to embrace three fields that are inter-related and overlap, but that can be defined as three distinct subjects. These are History of Science (and Technology), Philosophy of Science, and Sociology of Scientific Knowledge. History of Science and Philosophy of Science are disciplines that have had an existence for much of the twentieth century, but Sociology of Scientific Knowledge (and its predecessor, Sociology of Science) is slightly newer having come into existence in the 1960s. History of Science is and has been concerned with studying who has done science, what they have achieved, and how they have done it. Philosophy of Science looks at defining if and how Scientific Knowledge differs from other forms of knowledge; in other words, it is concerned with epistemology. Sociology of Scientific Knowledge grew out of looking at the social aspects of science (Sociology of Science), to looking at how the social aspects of science and scientists *create* scientific knowledge.

One of the common themes within these three disciplines is their treatment of the nature of Scientific Knowledge from the *intellectual* angle. In other words, a large proportion of

work in this area has concentrated on studying scientific theory; looking at scientific ideas, or the *beliefs* of scientists about the way the world is. There has been an emphasis placed on scientific ideas and theories as the repositories of knowledge about the world. The role of the practical in shaping knowledge about the world, and the nature of practical knowledge as a form of knowledge, was largely ignored by these disciplines until the 1980s. The development of the debate is summarised extremely well by Pickering in the introduction to *Science as Practice and Culture*.³

Since Kuhn⁴, Historians of Science have been prepared to use other philosophical and historiographical resources apart from treating the subject from an Empiricist⁵ perspective. The field has adopted some of the methodologies of SSK, especially in relation to looking at Science as a social phenomenon.⁶ Several of these authors have chosen to look at how the social relations of scientists have shaped their beliefs about the world. For example, the social conditions of Seventeenth Century England had a marked influence on the development of observation within a defined experimental space (the Royal Society), and the social status of the observers conferred legitimacy on their observations when they described these experiments to others.⁷ Some within the field of SSK, particularly those associated with Bath and Edinburgh, have chosen to study contemporary science in order to form theories about the nature of scientific knowledge.⁸ Part of their methodology has been to trace events as they happened. This has enabled them to use participant interview as a major resource. This is unavailable as a resource to historically minded Sociologists of Scientific Knowledge, such as Latour, who has studied Louis Pasteur.⁹

³ Pickering (1992b).

⁴ Kuhn (1962).

⁵ See, for example, Putnam (1978).

⁶ Examples of this are Shapin (1979), Shapin & Schaffer (1985), Schaffer (1989) and Smith & Wise (1989).

⁷ Shapin & Schaffer (1985).

⁸ See, for example, Collins (1985), and Bloor (1976). Edge & Mulkay (1976) differs in being historical.

⁹ Latour (1988).

The history of radar development has been well documented by participants¹⁰, and in the secondary literature in the form of reportative recounting of events.¹¹ The only exception to this is Susskind's analysis of radar as an example of simultaneous invention.¹² Certainly, no-one has attempted a history of radar along the lines that I have as far as I am aware.¹³ However the history of radar, and in particular that of microwave radar in Britain and Germany, is ripe for analysis under exactly the sort of theoretical framework that is used by historians and sociologists such as Gooding¹⁴, Pickering¹⁵ and Collins.¹⁶ They are amongst those who have looked very carefully at the role of human skill and agency in creating scientific knowledge. In the next four sections I shall explicate the form of analysis which I will bring to bear on the historical material I have gathered, and define it in terms of how it fits in with the wider literature of Scientific practice.¹⁷

1.2.1 Learning to "See".

Put in very basic terms, the whole point of a radar set is that it is a device that extends the human eye's ability; it allows people to "see" objects under conditions where the naked eye is unable to perform this task, as when the object is too far away, or obscured through fog or at night. The history of the development of radar is full of examples of the problems of trying to establish a link between an object in the world and a representation of it, most

¹⁰ See, for example, Batt (1991), Bowen (1987), Hanbury-Brown (1991), Hodgkin (1992) and Lovell (1991).

¹¹ The best example of this is Guerlac (1988). However, it is also interesting to note that Guerlac wrote this history as a contemporary observer, and made extensive use of interview - perhaps the first such history to do so.

¹² Susskind (1968).

¹³ Although Jeff Hughes' work involves the same people at the Cavendish in the 1930's, and Jon Agar also follows some of them as they moved into radio astronomy in the 1950's and 1960's, as does Edge & Mulkay (1976).

¹⁴ Gooding (1990a, b), Gooding (1992).

¹⁵ Pickering (1989), Pickering (1992a, b).

¹⁶ Collins (1985).

¹⁷ I am indebted to my supervisor for discussions that have clarified my methodological intentions.

usually on a cathode ray screen. This problem is not unique to radar; the difficulty of linking “representation” to “object in the world” has characterised the scientific enterprise since the Seventeenth Century.

This process has very often worked both ways. It is often the case in science that the representation is used as evidence that the object exists, or that an effect is “real”.¹⁸ For example, when Newton¹⁹ was arguing for his theory of white light by passing it through a prism to display a spectral image (a representation), his opponents were unable to “see” the same effect and blamed the glass in his prisms (this also underlines the difficulty of replication, which I will deal with in the next section). However, in the case of radar the stability of the object was not in question, it was the representation of it that needed to be fixed as not being an artefact of the apparatus.²⁰

The radar scientists and engineers had to ensure that they could link their representations to the objects they were trying to track. This involved them being able to communicate their private experience of that representation to their colleagues. They needed to persuade, and persuasion was not often a straightforward task initially, as I describe. I will highlight the structure of their experiments, showing that (to paraphrase Gooding) “observing anything but chaotic... behaviour depend[ed] on skilful manipulation..., and this [took] some time to acquire.”²¹

Observation using radar required several types of skill - it required the initial experimental and social skills of its inventors in designing it, building it, making it work and training others to use it. It then required other skills of its operators in order to turn it into an effective observational device. In the next section I will define the four types of skill that I have uncovered in operation with reference to other work on this area.

¹⁸ This is explored in Woolgar (1988).

¹⁹ See Schaffer (1989).

²⁰ This is similar to the history of the use of x-rays to represent the internal structure of the body. See Passveer (1993).

²¹ Gooding (1990b), p135.

1.2.2 Skill and Tacit Knowledge: Embodied and “Black-Boxed”

The second major theme that I wish to address is that of skill and tacit knowledge. This theme is concerned completely with science as practice: with what scientists do (their experimental skill), and with what they make (knowledge, in the form both of artefact, or apparatus, and effect, ie what the apparatus and the scientist produce together). As I explained in section 1.2, apart from a few earlier examples²² of interest in the practice of science, and what forms of knowledge practice could constitute, it is only relatively recently that there has been any work in this area. The majority of the recent interest has come from History of Science, and SSK.

One of the first people to challenge the view that scientific knowledge came only in the form of ideas or theories was Ryle. As he put it:

Theorists have been so preoccupied with the task of investigating the nature, the source and the credentials of theories that we adopt that they have for the most part ignored the question what it is for someone to know how to perform tasks.²³

He argued that there was no distinction between mind and body, but there was a distinction between “know-how” and “knowing that”.²⁴ What Ryle identified was that some forms of knowledge could be embodied, or could be classified as skills. These skills were the abilities of people to engage with the world through their actions, and had the further characteristic that they could in some cases be unarticulable. Polanyi characterised it thus:

²² See, for example, Ryle (1949) and Polanyi (1959).

²³ Ryle (1949), p28.

²⁴ Ryle (1949), ch 1.

[U]nformulated knowledge, such as we have of something we are in the act of doing,
is another form of knowledge.²⁵

Polanyi called this form of knowledge “Tacit Knowledge”. He was interested in tacit knowledge in the form of understanding, as “all human knowledge is... shaped and sustained by the inarticulate mental faculties which we share with the animals.” This knowledge is gained by experience, as “[the] body is always in use as the basic instrument of our intellectual and practical control over our surroundings.”

Someone who has studied the role of human skill in experiment is Harry Collins.²⁶ There are several points which I wish to draw out of Collins’ work in order to employ them in my own framework. Collins has concentrated on the issue of replicating scientific apparatus, and used this to show that in order for replication of effects to take place, then a transfer of explicit instructions is not sufficient. In Collins’ case study, a second scientific team is trying to build a TEA laser. Despite them concurring fully with the first team’s results, they are unable to get their apparatus to work until a member of the original team is able to bring with him his personal skilled knowledge of how to build TEA lasers. Collins shows how transfer of knowledge requires a social component. In this case, the scientist has embodied know-how, but this knowledge can be made explicit because it is of the form of knowledge that wasn’t required before the need to make explicit became necessary. This is one type of skill and tacit knowledge.

A further type of skilled tacit knowledge is embodied knowledge in the form of Ryle’s “know-how”: this type of skill could be thought of as manipulative skill. Gooding²⁷ has shown how Faraday learned skill which became “embodied”, when he learned the manipulative skills required to produce the rotation effect. However, not only did Faraday learn how to interact with his apparatus to produce an effect, but he also was able to

²⁵ Polanyi (1959), p12.

²⁶ See Collins (1985).

²⁷ Gooding (1990a).

produce apparatus in which the manipulative skill was removed. he had “black-boxed” those manipulative skills into an artefact which had “embedded” tacit knowledge. Gooding concentrates on the role of the world in prompting lines of investigation by examining the “fine structure of experiment”. There is a continuous process of learning by doing, such that theory and experiment are combined through human agency. The skill of manipulating experimental apparatus in order to see their effects sets up an interactive process such that what the experimenter thinks about what he is doing is shaped by the experience of doing, and also conversely in that the next steps are shaped by what the experimenter thinks when whilst doing. In this way, theory and experiment shape and modify each other.

When examining my work I will be looking for four things:

(i) Instances of embodied “know-how”, in the form of skilled interaction with apparatus in order to produce new effects, and also to interpret effects.

(ii) Instances of embedded “know-how”, in the form of artefacts which have taken non-explicit tacit knowledge and removed the necessity for an operator who has this knowledge, or skill.

(iii) Instances of embodied “know-how” that is made explicit after something fails to produce an effect, as in, for example, when apparatus is new and the skills are being learned, or when the apparatus is being replicated.

(iv) Instances where communication of knowledge takes place, either by transmission of embedded knowledge, or by embodied knowledge.

1.2.3 Communication

This final point brings me to the third theme, that of communication. Communication is a social process, and allows the transmission of knowledge. However, knowledge

transmission can take place in several different ways, and I will highlight some of the ways which it takes place and which I feel are of importance. For there to be an exchange of scientific knowledge, then there has to be some form of shared culture in order for that transmission to occur. For example, written or diagrammatical information can only be understood if the recipient is familiar with the language or the diagrammatical notation in which it is written or drawn. Furthermore, there has to be a shared scientific culture in order to facilitate the transmission of scientific knowledge.

I will be looking at communication on three levels, on the immediate personal level between people in a group, between groups within a country, and between different countries. The level of the research team provides examples of shared scientific culture, that show how at the lowest level, scientific knowledge is transmitted by personal contact. The individuals will be able to demonstrate their findings by teaching their peers how to see effects, and to produce them for themselves. Group members may also be able to provide assistance in the form of new ideas, or through their own particular skills. Such skills may be of the form of techniques necessary to manufacture particular items of apparatus, or they may be in terms of knowing how to produce the optimal set-up of a collection of apparatus through accumulated experimental skill: years of working in similar situations.

Transmission between groups is something covered by Collins²⁸, and Mackenzie & Spinardi.²⁹ Collins has argued (as I mentioned above), that replication of new scientific equipment and techniques *cannot* take place with just the transmission of algorithmic knowledge; it also requires the transfer of embodied knowledge in the form of skilled experimenters. They are able to provide tacit knowledge in the form of embodied skill in how to use the apparatus to get the required result, and they are able to make explicit knowledge that had hitherto been implicit because they had not required it. Mackenzie & Spinardi have modified this argument to say that replication of new scientific equipment and

²⁸ Collins (1985).

²⁹ Mackenzie & Spinardi (1994).

techniques *can* take place without the concomitant transfer of personnel with tacit knowledge, but that the replicators will most likely take as much time to make the replication as the original experimenter, despite having the knowledge of the outcome (as the original experimenters didn't). This is because they too had to learn how to use the apparatus; they had to build up their own tacit knowledge of how to operate the equipment, of how to engage with it physically to produce the correct effect.

I will be examining the case of the communication of knowledge between groups through the transfer of embedded knowledge in the form of equipment. In particular, I wish to see what the Germans found “transparent” about the British radar set they captured. In other words, were they able to replicate the set, and if they did replicate, did they fully understand its working? Were they able to deduce what they could learn about the new British apparatus, techniques and results from studying only the apparatus? Could they use this knowledge, if they gained any? This complements the work that I have already mentioned, by using Gooding's ideas of the tacit knowledge embedded in apparatus. This knowledge is “black-boxed” by engaging the world through experiment to the point where the original manipulative skill of the experimenter is no longer needed. Only interpretative skill is required to use it effectively, rather than manipulative skill

1.2.4 The Direction of Knowledge Transfer

This is a minor theme which I will make no more than a brief mention of. The main direction of knowledge transfer that I investigate is from Britain to Germany. This is rather a narrow view of the intelligence war fought between the two countries. Information flowed both ways, and both sides sought out as much as they could. They accomplished this in many ways; spies, equipment capture and radio frequency-monitoring were the most common.

The allies had the upper hand in terms of espionage by the fact that they did not occupy large tracts of land where they had to operate their secret equipment that had populations hostile to them, and ready to pass on information to the enemy. This was very much the case with Germany. Both sides regularly monitored the radio traffic (including radar) of the other side not only for information, but to see what frequencies were being used. Injudicious use of airborne radar could also be a give-away to the position or timing of operations. In particular, the Germans quickly employed a listening device for H₂S, called *Naxos*. I will draw attention to instances of knowledge transfer in both directions, as opposed to the main centimetre radar theme.

1.3 Resources

The resources and methods chosen by me to accomplish my aims include some that are traditionally associated with the historian, and some that aren't. The first sources of information on the topic that I used were books, and journals. Information contained in such a manner falls under several different types. Much of the published work on radar falls under the class of participant information of one sort or another. The journal material is much more limited. The second source available to me was archive material. This includes the reports and diagrams produced at the time, and in some cases laboratory note-books too, and constitutes the major primary source.

Primary information can also come from interviews with participants, a method not used as much by the historian as by the sociologist. This is due largely to the obvious necessity of participants being alive in order to conduct interviews with them. In this case, the events are still sufficiently recent for there to be some participants still alive. This method of research was interesting in that it offered possibilities for corroboration that are not usually available. I found that three out of my four interviewees had already written up their experiences in the

form of memoir or histories. The interview allowed me to clarify some areas of ambiguity, and ask for their impressions on events. The fourth interviewee discussed his laboratory notebook and impressions with me. In addition, one of the interviewees took me around key sites in the development of the first British microwave radar. As nothing remains of the huts where the experiments took place, he was able to show me the exact locations of buildings, and the places where some of the experiments occurred and the landmarks used for calibration of equipment.

In all these cases of interview my experience has been very similar to that of Frederic L. Holmes, who used interviews of Hans Krebs in later life in conjunction with Krebs own notes and papers from the time he was doing his major work. Krebs' later thoughts are very different to those when he was doing his early work; his early uncertainty is replaced by a later conviction about how things appeared.³⁰ This process goes on between the original source material and my interview material, and I make mention of it where it occurs. As Gooding points out, this allows us a window into the process of reconstruction that a scientist uses, not only when first performing the work but also in later life.

I examine a related theme in Appendix A, that of the difference in account afforded by the difference in perspective of participants according to their level of competence and their social position. In particular, I compare the account of one of the scientists, Lovell, who later achieved high status in his field of radio astronomy, with one of his technicians, Batt. Batt was not a research scientist like Lovell; he was a trained technician. He had some skills which Lovell did not, and vice versa. His different social standing in terms of his position in the hierarchy of the laboratory meant he saw events in a different way to Lovell. This difference in perspective has become visible when one compares the two accounts. The two differ in what they remember with most clarity, and in how they describe events where each was a participant.

³⁰ Gooding (1993).

1.4 Outline of Thesis

The main body of work divides into two sections. The first section is concerned with British developments. The second section examines the German developments, draws together the findings from the previous section and that section, offers an analysis of the material, and presents some answers to the questions outlined above in the light of the analysis.

Within the first section, there are four chapters. Chapter two looks at the development of radar in Britain before the centimetre revolution. It explores the technical and political background that gave the conditions for this development, and the subsequent directions taken that lead to the realisation that centimetre waves were desirable. It shows how initial AI success was achieved using small highly skilled teams, and how these teams came to be broken up and marginalised. It points out what these radar-building skills were, and how they would subsequently be useful. I also make mention of some of the other outcomes that could have occurred without the particular chain of events that lead to the decision to press ahead with centimetre research. This is interesting in the light of the Germans' decision not to go down this particular road.

Chapter three presents the development of centimetre components in Britain, with some collaboration from France and America. It looks at the state of knowledge that existed about the subject before any research was undertaken with the specific development of radar in mind. It examines where and when the significant advances in component design were made, especially the cavity magnetron, and again shows how small, highly-skilled teams operated in concert with each other to produce semi-manufactured components that were absorbed into the fledgling radar programme. These first two chapters rely mainly on secondary material. The next chapters are based more on primary material.

The fourth chapter explores the different ways in which the problem of centimetre radar was tackled, by GEC (General Electric Company) and by AMRE (Air Ministry Research

Establishment), initially separately but later in unison. GEC developed a 25cm system which was working shortly before AMRE were able to produce anything on 10cm with the cavity magnetron. It shows how the researchers suffered problems of replication, experimental skill and team-work in constructing the first operational centimetre AI.

The fifth chapter follows the development of the H₂S blind-bombing and navigation system. It looks at the political pressures for such a system, the realisation that one was possible, and the difficulties experienced in producing anything workable and worthwhile. It also notes the difficulties experienced by the Americans in replicating British results despite readily available information. The bounds of the chapter extend to the system that was used operationally and captured by the Germans, in February 1943.

Similarly, the second section, which is concerned with German developments, contains three chapters. Chapter six explores the development of German radar prior to the discovery of the British centimetre set. It follows the aims of the German researchers and designers, and looks at the reasons why research into the centimetre waveband was largely ignored.

The seventh chapter looks at how the Germans came to grips with their discovery. What difficulties there were for them in teasing out the secrets of the knowledge embedded in the set, and how these were overcome. How did they reorganise their research, and what impact did this discovery have? Were the Germans able to replicate British/American centimetric knowledge successfully? Did they achieve this more speedily knowing that the outcome of such research was possible?

In the final chapter there are two aims in mind. The first is to compare developments in Britain and Germany, before and after H₂S. What lead British research to take a different direction to German? How well did the Germans "catch up"? The second aim is to examine the implications of this story for the understanding of technological innovation and proliferation, and the way knowledge is created and disseminated. Lastly, I offer some

conclusions from the work, and gives some pointers to future research that may be undertaken either from this work, or using its findings.

British microwave radar research followed patterns that will be recognisable to anyone familiar with Collins' and Gooding's work. The successful, speedy research especially in Britain was conducted in atmospheres of co-operation between individuals and teams, with great ease of communication between these individuals and teams. The personnel involved had similar backgrounds, and/or had acquired great practical skill in dealing with electronic components and apparatus. They also usually came from backgrounds that were unconventional in the terms of the field, such as physics, or even biochemistry. There was also much co-operation between the research teams, the military and the manufacturers. Where this broke down, or where persons or teams were isolated, progress was slow, and the "wheel was often reinvented". Links provided the source for many instances of "serendipity".

German research was much more fractured and suffering from the kind of bureaucratic interference that British research to some extent managed to throw off. The plus side was that their equipment was well engineered, robust and easy to service. Later on, despite efforts to reorganise when the extent of the Allied microwave revolution became clear, research was still split around institutions in physically different places. To what extent there was co-operation between these institutes is unclear. The Germans took roughly the same time to develop experimental centimetre sets of their own as their British counterparts, so it would seem possible that at best, fore-knowledge of the success of microwaves provided no advantage. In some cases it could be argued that given Germany's particular situation in the later stages of the war centimetre research was a diversion as the resources employed into it would have been better used elsewhere.

Chapter 2: Pre-Microwave Radar

2.1 Introduction

The main body of this thesis is concerned with how the British developed centimetre radar, and then how the Germans copied this discovery. However, before I go on to look at the British centimetre radar developments in chapters 3, 4 and 5, and the German developments in chapters 6 and 7, there is something that needs to be done first. That something is to tell how radar came about, and what the British had learned about how to construct it, develop it and use it before its perceived limitations meant that centimetre wave radar came to be required and developed

The date generally accepted by historians for the beginning of radar in Great Britain is February 26th 1935. On this day, in a van in a field near Weedon in Northamptonshire, "beats" (a form of fluctuating signal) were received from a Heyford bomber flying through the beam of the BBC Empire short-wave transmitter at Daventry. The significance of this event lay not in the effect produced, for such an effect had been seen, and noted, before, but in that this demonstration had been arranged for a purpose. That purpose was to persuade the Air Ministry, and more specifically Sir Hugh Dowding, head of Fighter Command that they should provide funding into researching a means of early warning, based on the reflection of radio-waves. As such it was successful.

Radar is a means of detection using radio waves. That it was developed at this time in particular, and at all depended on several factors: the recognition of the reflective and detective properties of radio waves, the recognition of the detectability of aeroplanes and other objects by such methods, and the need for such a method of detection.

Any attempt at the writing the history of a scientific or technological artefact must involve decisions about where to begin the account, what is included in it, and what is left out. Any start-point is in some senses an arbitrary decision, for in the history of science the decision of what constitutes a key event is essentially a retrospective one.

This chapter will examine the genesis of radar, primarily in Britain, up to the development of the first airborne apparatus. This particular application was the main spur

for the development of microwave radar in this country, so it will serve as a background chapter for those events, chronicled in Chapters 3, 4 & 5. However, the story is also an international one, so I will also mention episodes that occurred in other countries. Events relating specifically to the development of pre-microwave German radar will be covered in Chapter 6.

I will look at the radio background of radar, in terms of commercial industry and research into atmospheric composition, where many of the skills and techniques used in radar research were perfected. The political situation in Britain in the early 1930's, in particular the fear of air attack from a resurgent Nazi Germany provided the impetus for using science to investigate the possibility of air defence against bombers. Sir Henry Tizard, Rector of Imperial College, scientist and former test-pilot, headed a government committee tasked with finding a solution to this problem. This committee was instrumental in setting up the research establishment, first at Orford Ness, and later at Bawdsey Manor, that would become known during the war as AMRE and then TRE.

Research begun in 1935 led by 1936 to the development of a system that enabled aircraft detection to accuracies of around 5 miles. Experience garnered with this system by regular exercises in conjunction with RAF fighter squadrons led to the development of a co-ordinated fighter-control network, using information provided by radar stations located in a gradually extending chain around the coastline. This type of radar became known as Chain Home.

When Tizard was satisfied with progress toward the problem of daylight detection, he started to press in 1936 for an airborne system that could detect aircraft at night. His reasoning was that, if daylight raids could be detected easily, then the bombers would be forced to operate in overcast or night-time conditions. Therefore some other method of detection more accurate than Chain Home would be required. He set up a small team under the leadership of Dr E.G.Bowen to look at this problem.

Over the next three years Bowen's team went a long way towards developing skills, techniques and apparatus peculiar to airborne operation, where the difficult and cramped conditions caused difficulties in addition to the normal experimental problems encountered in a warm, stable, spacious laboratory. Events of 1939-40 described here, and in Chapters 3

and 4 effectively sidelined Bowen, and the many skills he and his team built up were nearly lost. Fortunately they were able to pass many of these on to the new recruits such as Lovell and Hodgkin, but later in the war their experience was missed. I wish to examine the nature of Bowen's team and their investigations in relation to the questions posed in the introduction, and to show how his was a highly skilled, close-knit team that very similar to those which characterised the centimetre teams of the war. Their expertise was under-utilised due to political tensions within the establishment. Nevertheless, they managed to develop a crude form of Airborne Interception (AI) radar by the outbreak of war, along with a ship and submarine system ASV (Anti Surface Vessel). It was Bowen who was one of the main protagonists behind beginning centimetre research when he realised that his own remarkable 1.5m AI would still have operational defects that would limit its range to the height of the aircraft above ground.

2.2 Political and Technical Background

2.2.1 The Beginning

Radar is a means of detection using radio-waves. The background to the development of radar can be considered to consist of four parts. Firstly, the recognition of the reflective properties of radio-waves. Secondly, the development of components and technologies adaptable to radar purposes. Thirdly, investigations into methods of detection of aircraft and other objects. Lastly, the impetus to recognise, push for and develop radio-wave detection methods into working systems. I shall give an account of each part.

Maxwell's mathematical theory of electro-magnetic waves predicted the existence of undiscovered waves whose properties would be similar to those of light. These properties were that the waves would have a velocity equal to that of light, and that they would be "reflected from conducting surfaces and refracted by dielectrics according to the classical laws of geometrical objects".¹

¹ Guerlac (1988), p33.

Such waves were first produced by Heinrich Hertz in experiments conducted during 1887-88. Once he had succeeded in generating and detecting radio waves, he began a series of experiments that showed that these new waves behaved in the same manner as light. Hertz's first apparatus used a spark gap transmitter that produced waves of 10m in length. Subsequently he managed to generate waves as small as 66cm. His experiments confirmed that the waves had light-like properties, such as reflection, refraction, polarisation, shadow-casting and diffraction.

In the following decade other researchers demonstrated the optical properties of what are now termed microwaves, or electromagnetic waves of lengths of around 10cm to millimetres. For example, the Russian Peter Lebedev, using 6mm waves, showed double refraction in a crystal. This had not been possible with the longer waves of Hertz's equipment.

It was not long before people realised the possibilities of the "Hertzian Waves" for "wireless" telegraphy. Marconi's initial experiments in 1896 were made using 1m waves with a parabolic reflector behind both the transmitter and the receiver. However, the technology of the day meant that it was only possible to generate longer waves of sufficient power for communication purposes, and this led to the virtual abandonment of short-wave work around the turn of the century.

2.2.2 Radio Technology

The expanding field of radio, or wireless, led to the taking of a number of steps along the road to the development of radar. These steps included several devices that, with hindsight, could be viewed as radar apparatus. Radio, and, to a lesser degree, television, were the providers of the basic components of radar. In this section I shall outline the development of the components for radar, and the ways in which some of the techniques used were invented and developed.

The main requirement for a radar set, when the possibilities of the technique were eventually realised, was for a valve that could produce very short radio waves at very high

had the embodied knowledge of an unarticulable kind, of working with radio equipment. This is the kind of experimental skill that can be “black-boxed”, but also assists the experimenter when they are working in terms of helping them to make decisions about how to progress. When the war finally came, these persons were among those recruited into radar research.³

The utilisation of long waves for radio communication at the beginning of the Twentieth Century was largely due to fact that at this time it was only possible to generate the high transmitter power needed for broadcasting at these longer lengths. Due to the almost exclusive utilisation of wavelengths of the order of hundreds of metres, the optical-type effects that were more readily observed with very short waves passed out of the everyday experience of scientists and engineers. Despite this lack of exposure to very short waves, which might lead one to expect a lack of interest in experimenting with them, their reflective properties were first used in a detection device in the first decade of the century.

Several historians⁴ have identified the anti-collision device of Christian Hülsmeyer, patented in 1904, as the earliest example of a radar. However, as it was not a pulsed device, others do not classify it so, despite other similarities. Hülsmeyer was a young engineer who originally trained as a teacher. He had used his training as an opportunity to repeat Hertz's experiments, as he was interested in Physics. He became involved in the idea of an obstacle detector for ships after witnessing the grief of a mother whose son had been drowned in a collision at sea. This prompted him to search for a means of avoiding such collisions and unnecessary deaths.

For its time, Hülsmeyer's apparatus was remarkably advanced, and contained several innovations that would later become standard in radar sets. Principal of these was the use of a method of shielding the receiver from the transmitter. This also discriminated between the signal received from its own transmitter, and that from another transmitter. The apparatus operated on a wavelength of 40-50cm, and had a range of about 5km. The transmitter and receiver both had directional aerials bearing a strong resemblance to those devised by Professor Yagi in the late 1920's (such "Yagi arrays" are nowadays seen on many roofs in

³ Guerlac (1988), pp36-40.

⁴ See, for example, Swords (1986) pp43-5, Pritchard (1989) ch1 and Guerlac (1988) p41.

distance further than the direct (ground) wave would travel. They assumed that the reflection occurred from a layer of charged ions. Despite this early interest, little experimental work was done on investigating the phenomenon until the early 1920's. The success of amateurs in using short-wave transmissions for long distance communication, and the unusual associated effects, revived interest in it.

In 1924 the so-called "skip distance" (see Fig 2.1) was discovered by amateurs using 100m transmissions. This was a distinct gap in the distance away from the transmitter where transmitted waves could be received, arising from the different distances travelled from the ground (direct) and sky (reflected) waves. Discovery of this phenomenon opened up investigations into the properties of the layer that was believed to cause this reflection. In Britain Appleton and Barnett were the first to work on this area, using a frequency-change method similar to that later employed by the Russian Geodesy team. In December 1924 and January 1925 they calculated the height of the "Kennelly/Heaviside Layer". This work was also taken up in America by two researchers called Breit and Tuve, who were the first to employ a pulse technique making these measurements. They calculated the height of the layer to be between 50 and 130 miles. They thought that their initial results may be due to the Blue Ridge Mountains, which were also at about that distance from their transmitting and receiving apparatus. This is interesting in the context of later work on radar, when the experimenters were still trying to map what they "saw" with their apparatus to an object in the outside world. This is a perennial problem in science, and one that has occurred in many other instances as I have described in the Introduction. I make further mention of the problem in a radar context in chapters 2 and 4.¹⁰

Subsequent to its publication, Breit and Tuve's pulse method was adopted widely, and the British team abandoned their continuous-wave/frequency-change method. The interest that this work generated led to the amassing of a large body of research aimed at exploring the upper atmosphere using radio methods. Subsequent work during the late 1920's and early 1930's refined the techniques to get shorter and clearer pulses, and introduced the cathode-ray oscilloscope as an indicator. Amongst those working on this area in Britain was Robert Watson-Watt, who was responsible for coining the word "Ionosphere" to describe

¹⁰ Guerlac (1988), pp50-3.

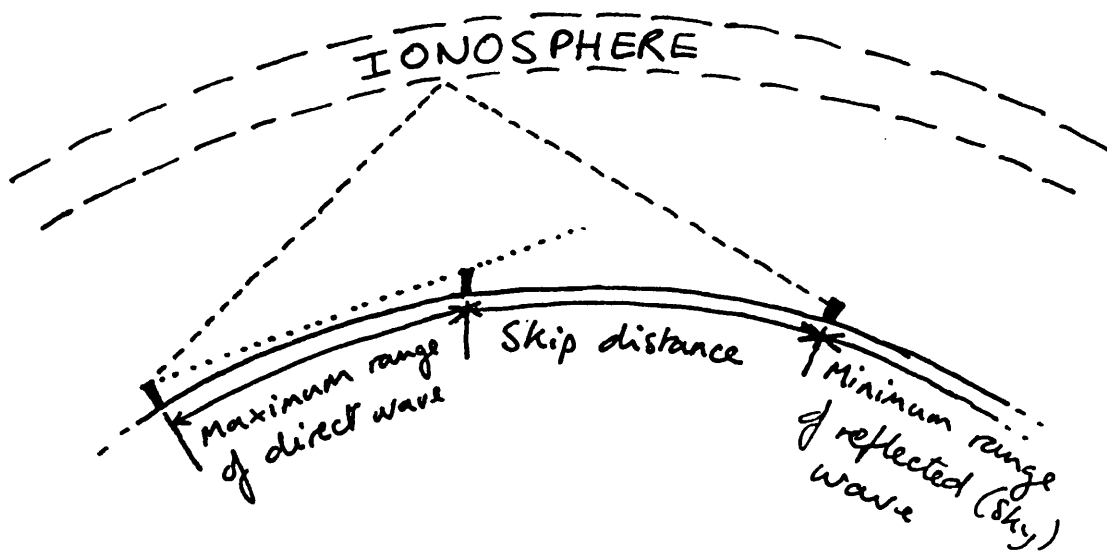


Figure 2.1: Sky wave, ground wave and skip-distance. This diagram shows the area where no signals can be received (skip distance), because the range is too great for a direct wave from the transmitter (ground wave), and too short for the reflected wave from the ionosphere (sky wave).

the various reflecting layers in the upper atmosphere.¹¹ It was his familiarity with pulsed radio distance-measuring techniques that led to Watson-Watt suggesting this method for the location of aircraft when he was asked about the problem in 1935.

Lastly it is worth mentioning that the effects of aircraft interfering with radio beams were published *twice* during the early 1930's. Prior to these two instances of publication, in January 1931, W.A.S. Butement and P.E. Pollard of the (British) Signals Experimental Establishment submitted a proposal to the War Office. After the War Office rejected it, Butement and Pollard also submitted it to the Admiralty, who also rejected it. The Air Ministry were ironically *not* approached, for after two rejections Butement and Pollard were not inclined to seek a third. The Air Ministry had the greatest need for such a system, and if they had heard of Butement and Pollard's work it might have given them three years' start. Their system was, in Clarke's words, "the world's first rudimentary radar proposal."¹² They proposed the detection of ships from ship or shore using a transmitted radio pulse on a wavelength of around 50cm. They suggested using a rotatable beam, with directed antennae to ensure the beam was narrow. Before their work was halted due to the rejections they managed to perform some basic experiments that obtained reflections from stationary objects, which indicated some promise for the idea.

In December of 1931, Post Office engineers at their Research Station at Dollis Hill in London were experimenting with a short-wave communications system. They noted some unusual effects whenever aircraft flew through the beam. These interesting results were written up in a report dated 3/6/32. They explained that the aircraft set up a pattern of "beats" interference (much the same as that later used in the so-called "Davenport Experiment"), and they even attempted to correlate the frequency of the beats with the aircraft's speed. Independently, in January of 1933 workers at the Bell Telephone Laboratories in the United States published more detailed observations of the same effect that noted that it occurred even when aircraft were out of sight.¹³

In this section I have indicated how the technology and skills that came to be used in radar arose. The apparatus and components were high-power, short wave valves for pulse

¹¹ Guerlac (1988), p52; Watson-Watt (1957), p92.

¹² Clarke (1965), p114.

¹³ Watson-Watt (1957), p94.

operation, and CRT displays. The skills were those necessary to build and operate transmitters and receivers utilising these components. These skills and equipment came from a mixture of scientific and commercial interest, aided in some cases by military pressure. However, reflection of radio waves by objects remained solely an interesting and sometimes irritating phenomenon until 1934. This attitude changed due to concern about how to detect bombing aircraft, and the belief that a new method of detection could be found by using science and scientists to investigate the problem. The political pressure that led to scientific interest in this phenomenon being revived and actively pursued, will be recounted in section 2.2.4.

2.2.3 Other Detection Methods

Just as there were several different types of object which scientists and the Military were keen to detect, there were also several different methods tried for detecting them before the pulsed radio-wave technique was almost universally adopted. These other methods fall broadly into three types: utilisation of sound, infra-red, and ignition-noise.

Sound locators were developed as aircraft detectors during the First World War and were also used during the 1920's and 30's. The first locators were little more than large ear trumpets; later ones were much bigger. The major problem with sound location was the relatively slow speed of sound (approximately 700 mph; radio waves travel at the same speed as light: 186 000 mph) in relation to the speed of the aircraft (2-300 mph at this time). The problem was that because the speed of the aircraft noise travelled only 3 times as fast as the source of that noise, by the time the noise arrived at the listener, the aircraft had already moved a considerable distance nearer. The faster the aircraft, the greater the distance it had moved and the less time afforded by warning apparatus. By the early 1930s aircraft speeds were such that the utility of sound locators was negligible.

Nevertheless, a programme for building large, concrete sound locators around the South East coast of England commenced in 1933. A large concrete sound-mirror was built on the edge of Romney Marsh in Kent pointing in the direction of Paris, as France was the

European nation with the largest bomber force at that time and therefore perceived by the British as their biggest threat. This, incredibly, was despite the recent seizure of power in Germany by Hitler. As Watson-Watt later put it, "Reinforced concrete is rudely uncompliant to the winds of diplomacy".¹⁴ Furthermore, this mirror was next to useless. It would pick up nothing unless the aircraft was almost directly in line with the focal axis of the mirror, and even then any sound could be easily masked by other noises, such as motor boats, cars, or the wind. However, "this unpromising work went on because there was apparently no other hope".¹⁵ Even after the eventual development of the coastal radar chain, sound locators continued to be used inland by the Royal Observer Corps, as there was no other method in place for detecting aircraft once they had crossed the coastline (the radar chain only pointed outwards from the coast).

Another method tried as a means of detecting aircraft relied on emissions from them. The ignition systems of petrol engines generate electromagnetic radiation. This occurs because of the high-voltage spark that is used to ignite the air-petrol mixture. During the late 1920's the French considered using this effect as the basis of a means of detection, until they realised that efficient screening of the emissions could eliminate their usefulness.¹⁶

Investigations into the military usage of infra-red radiation (another detectable emission) began during the First World War. The first experiments were conducted into detecting people, ships and vehicles, and met with a reasonable degree of success. Infra-red detection of aircraft was limited by the sensitivity of the available receivers, and by the inability of infra-red to penetrate through clouds (it is absorbed by water molecules). Nevertheless, during the late 1930s R.V.Jones, a young British graduate student, pursued the possibilities of this phenomenon as a means of detection, at the Clarendon Laboratory in Oxford. The Laboratory head, Professor Lindemann (later to become Churchill's scientific advisor), strongly supported this line of research, especially as a means of detecting aircraft from other aircraft at night. Lindemann was a noted "hawk" of the period, as I recount in the next section. Writing in 1972, Jones claimed that:

¹⁴ Watson-Watt (1957), p80.

¹⁵ Rowe (1948), p4.

¹⁶ Swords (1986), p48.

On April 27th 1937 I made what appears to have been the first flight in which an aircraft was detected by another in flight by infra-red. The range was modest - only 500 yards - but the limitation was not due to the detector but to the... transformer...¹⁷

Despite this interest, Lindemann was notoriously antagonistic toward radar. Eventually, this led to personal enmity between him and Sir Henry Tizard, the chair of the committee formed to investigate Air Defence. This committee eventually chose radio-wave detection as the best option for detecting aircraft using scientific methods. It is possible that infra-red detection could have been useful in the context of night-fighting, as a means of detecting other aircraft. However, by that stage (1936) Lindemann's stock was so low that radar was the only option considered by Tizard's committee (see section 2.1.4).

The potential of these other methods, with the possible exception of infra-red, was exhausted by the time anyone decided to investigate seriously the possibilities of scientific research into detecting aircraft. That is one of the main reasons why radar came to be accepted so wholeheartedly and so quickly as the best method of solving Britain's air-defence problems. This was despite recognition (in some quarters) of the defects of the technique, such as its susceptibility to jamming.

2.2.4 Political Background

Edgerton has argued recently that Britain has long had an appreciation of the value of air-power, and its own vulnerability to it.¹⁸ The British State saw air power as a technological alternative to having to use large numbers of personnel, which the economy could not afford, stretched as it was with commitments to Imperial defence. Edgerton argues that Britain was *not* inadequately equipped when war broke out (as the popular myth would have it). Rather, the Air Ministry and senior members of the Government had an

¹⁷ Swords (1986), p51, quoted from Jones, R.V. (1972) "Some turning-points in infra-red history", *The Radio and Electronic Engineer* 42 pp117-126.

¹⁸ See Edgerton (1991) for an aeroplane-orientated radical reinterpretation of Britain's rôle as a technological and military power.

appreciation of the possibilities of the aeroplane throughout the 1930's. By appreciating, and even exaggerating the threat that air power posed to an industrial society the State recognised the need to prepare for air defence. It was in the light of such appreciation that Stanley Baldwin made his remark on 10 November 1932, that "The bomber will always get through".

Britain was the only country to emerge from the end of the First World War with an independent Air Force. Shortly before the end of that war the newly formed Royal Air Force¹⁹ founded a strategic bombing force in order to attack German industrial targets. During the late 1920's and 1930's the Royal Air Force was often used for policing duties on the Empire's frontiers, especially in Iraq²⁰, where the potential of using aircraft to exercise military (and political) power in a cheap and effective fashion was first recognised. As such, Britain was unique amongst World powers in putting such official faith in aircraft, and in perceiving the threat from them.²¹

By 1934 Hitler was in power in Germany, and it became increasingly obvious that he was rebuilding the German Air Force. This was contrary to the terms of the Versailles Treaty, the peace accord from the First World War which forbade Germany to have any Air Force whatsoever.²² There was an increasing realisation amongst politicians and the military that the British Isles were becoming ever more vulnerable to air attack. The speed of bombers relative to fighters was increasing, making them more difficult to shoot down (even if they could be found). The nature of the geography of the British Isles meant that this threat was keenly felt. Enemy aircraft could only be spotted within close proximity to the coast. Therefore, with nowhere more than 70 miles from the coast, and most targets less than this, defending fighters would not have enough time to take off and climb to meet

¹⁹ The Royal Air Force was formed by amalgamating the Army's Royal Flying Corps and the Navy's Royal Naval Air Service. It was done despite heavy opposition by the two older services, and for some years after the end of the war lived in danger of being returned to its former masters.

²⁰ The commander of one of the squadrons engaged in these activities was Arthur Harris, who became a strong advocate of area strategic bombing and Head of Bomber Command during the second half of the Second World War.

²¹ Edgerton (1991) pp16-21. This view is also confirmed by Gunston (1986), pp32-3.

²² The Germans had for some time been training pilots in places such as the Soviet Union, also ostracised during the 1920's. There was also a proliferation of gliding schools where many Germans learned to fly, no doubt in preparation for the day when Germany was allowed or decided to have an Air Force again.

raiders after they had been reported crossing the coastline, leaving the whole country vulnerable.²³

Further anxiety was caused by the Summer Air Exercises of 1934. A series of night "attacks" were conducted against London and Coventry. RAF bombers were sent to probe the air defences of these targets. In the first attack the Air Ministry building was deemed successfully "destroyed". More than half the "attacking" aircraft reached their target. It looked as though Baldwin's prediction about the bomber always getting through was tellingly accurate. At this time the main method of early warning against air-attack was a large concrete mirror (the sound locator mentioned in the previous section) designed to pick up the sound of approaching aircraft. It was starkly clear that this method was inadequate for the demands of locating the rapidly improving bombers of that time.

In 1924 the Air Ministry had appointed H.E. Wimperis as its first Director of Scientific Research, after Henry Tizard (of whom more will be said later) refused the post. Wimperis graduated in mechanical engineering before the First World War, during which he joined the Royal Naval Air Service. He helped to set up a research laboratory at Imperial College in 1917 and remained there after the war until appointed to the Air Ministry. At the same time as Wimperis' appointment, A.P. Rowe, a civil servant who was a physicist by training, was appointed as his Personal Assistant.²⁴

It was in the atmosphere of resignation towards the threat of air defence that was current in September 1934 that Rowe, on his own initiative, undertook a review of Air Ministry files relating to air defence; he found 53. This was by no means encouraging, as "there were at that time several times 53 *thousand* files in Air Ministry on other topics; and many of the 53 [he found] contained mere brief letters or one-page memoranda."²⁵ Faced with such a dearth of thinking on this topic, writing in 1948 Rowe summarised the situation facing him thus:

It was clear that the Air Staff had given conscientious thought and effort to the design of fighter aircraft, to methods of using them without early warning and to balloon defences.

²³ Clarke (1965) pp105-6.

²⁴ Swords (1986) p84; Clarke (1965) p66, 69.

²⁵ Gunston (1976) p37.

It was also clear however that little or no effort had been made to call on science to find a way out. I therefore wrote a memorandum summarising the unhappy position and proposing that the Director of Scientific Research [Wimperis] should tell the Secretary of State for Air [Lord Londonderry] of the dangers ahead. The memorandum said that unless science evolved some new method of aiding air defence, we were likely to lose the next war if it started within ten years. Unfortunately, I was not clever enough to think of a new method.²⁶

Wimperis and Rowe were not the only ones being galvanised into action by the perceived threat of air bombardment. Within the Royal Air Force, a sub-committee of the Committee of Imperial Defence (CID) was formed under the then chief of British Air Defence, Air Marshall Sir Robert Brooke-Popham. In addition, from within Parliament Winston Churchill (at this time enjoying one of his periods in the political wilderness) agitated for something to be done to rectify the complete lack of provision for air defence against bombers. Churchill relied heavily on the head of the Clarendon Laboratory in Oxford, Professor Lindemann, for scientific advice. With Churchill out of favour, Lindemann's influence was low, a situation that later changed greatly with the fortunes of his patron.

Following the advice of Rowe's memo, in October 1934 Wimperis contacted Professor A.V.Hill of University College, London. Hill was a physiologist by profession, but he had worked on armament research during the First World War. Wimperis wished to discuss the possibility of using electromagnetic radiation to disable or kill people on board aircraft. The so-called "death-ray" was a popular contemporary idea and Wimperis decided to find out whether it belonged firmly within the boundaries of science fiction. Proposals for such a device would arrive regularly on his desk, usually accompanied with polite requests for large amounts of money with which to commence or continue further work. The two met on 15th October to discuss the problem of air defence, and Wimperis left the meeting intending to put a proposition to the Air Council.

The next step occurred on 12th November when Wimperis finally sent his proposition to Lord Londonderry. The document suggested that a committee be formed to study the

²⁶ Rowe (1948) p4.

problem of Air Defence from a scientific perspective. He proposed that the committee consist of himself, Hill, Professor P.M.S. Blackett (a physicist who had served in the Navy) and Henry Tizard, rector of Imperial College, Chairman of the Air Ministry's Aeronautical Research Committee, and a former test-pilot. Rowe would act as secretary. All these men were also personal friends of Wimperis. Furthermore, the make-up of the committee fitted it to perform equally well within the interleaved but distinct worlds of academia, government and the armed forces.

Tizard was formally asked to chair the committee on 12th December. Its terms of reference were to be "to consider how far recent advances in scientific and technical knowledge can be used to strengthen the present methods of defence against hostile aircraft."²⁷ Tizard then wrote to the other members of the committee to ascertain their views, and arranged a meeting for 28th January 1935. The committee was named the Committee for the Scientific Survey of Air Defence (CSSAD), and was referred to informally as the Tizard Committee.

However, in early January and prior to this first meeting Wimperis contacted another friend, Robert Watson-Watt, who worked at the Radio Research Board at Slough, in order to question him too about the possibilities (if they existed) of the "death-ray". Watson-Watt put the proposals to one of his junior researchers, A.F. Wilkins, who concluded that radio-waves could not be generated at sufficient power or wavelength to have any appreciable affect on the structure of an aircraft or its occupants. However, as Watson-Watt pointed out at the conclusion of his report:

[A]ttention is being turned to the still difficult but less unpromising problem of radio-detection as opposed to radio-destruction, and numerical considerations on the method of detection by reflected radio waves will be submitted if required.²⁸

²⁷ Clarke (1965), p112.

²⁸ Watson-Watt (1957), p83. At this time Robert Alexander Watson Watt was just plain Mr Watt; after being knighted in 1942 he changed his surname to Watson-Watt. The quote also gives an example of his rather flamboyant and wordy prose-style.

Wimperis passed on the findings of this report to the first meeting of the CSSAD. The committee followed up by requesting further details, as Watson-Watt suggested. Watson-Watt set a further problem to Wilkins, based on the amount of energy an aircraft would re-radiate if it was assumed to behave like a 25m horizontal dipole. The results of his calculations were submitted to the committee in a letter dated 14th February. In it were contained not only the calculations that showed that detection was a distinct possibility, but also a clearly articulated vision of how detection could be incorporated into a defence system. This was a very important document as it shaped British thinking about radar for the next few years and culminated in the Chain Home early warning radar. Gunston's analysis gives some idea of the effect of Watson-Watt's report:

In this document - as important to British history as Magna Carta - [Watson-Watt] described not just the basic underlying principles of radar, but also the way a complete British defence system should be constructed, approximately how it should perform, and how the complete system - with communications to plotting centres and to fighter pilots, I[dentification] F[riend] or F[oe] and even enemy anti-radar countermeasures - would eventually function. Not the least thing about it was its air of total authority. Watson-Watt, in precise and official language, was describing techniques and often even existing hardware that he and his colleagues had devised, built and used. This was no 'mad inventor' with a harebrained and unexplainable secret, but a mature engineer with a proposal that was wholly valid, wholly explainable and capable of swift verification. By no means least, it was enormously encouraging; Watson-Watt himself promised 'The amount of reflected energy you will get back is amazingly big'. How the whole thing seemed to an Air Staff previously barren and desolate of ideas can be imagined.²⁹

Whilst perhaps over-exercising the benefit of hindsight, it does show that the important thing about Watson-Watt's report was that it considered all the implications of such a method of detection. Such thought for radar strategy was a part of British thinking right from the beginning, a situation different from Germany and all the other countries engaged in radar research.

²⁹ Gunston (1976), p39.

These positive findings were discussed at the second meeting of the Tizard committee was held on February 21st. Wimperis had by this date already suggested to Air Marshall Sir Hugh Dowding³⁰, the Air Member for Research and Development, that £10,000 be spent immediately on investigating the possibilities of Watson-Watt and Wilkin's paper. Dowding, in keeping with his reputation for caution, insisted that they carry out preliminary tests, which Wimperis duly arranged. Watson-Watt's initial idea was to use his own ionospheric research transmitter at Slough, but Wilkins considered that it would be impossible to modify in the time available. He recalled that the BBC transmitter at Slough worked on a wavelength of 49.8m, almost exactly right to give maximum re-radiation from the 25m aircraft wingspan.

They set up their experiment on the 25th February, and carried it out on the morning of the 26th. From a technical perspective the demonstration was merely to show that aircraft reflected sufficient amounts of radio energy to make this method a possibility for detecting aircraft. Wilkin's calculations showed that it *was* possible, but as Watson-Watt said in the report: "It turns out so favourably that I am still nervous as to whether we have got a power of ten wrong, although even that would not be fatal."³¹ He also pointed out to Dowding the uncertain nature of such an experiment, but qualified these uncertainties in his favour, as he related in 1957:

[T]his is a game which I cannot and will not play unless I am allowed to write my own rules. They are quite simple. If I score, I have won. If I don't score, 'it don't mean a thing'! If we don't get the indications we expect, it will not be because we are wrong in our theory or seriously wrong in our rough figuring. It will be because we have 'lashed up' a rough equipment, made up of parts meant for other purposes, set up a miserable strand of wire as an aerial, picked an unsuitable site, mis-estimated the strength of the Daventry beam, misinstructed the pilot, who can't be allowed to know why he is patrolling a dull and

³⁰ Dowding's nickname was "Stuffey" - a reference to his abrupt manner. This way of dealing with others made him no friends in high places, and later led him to be treated shabbily. He was due to retire in 1939, but his appointment was extended for a year, a period which as it happened covered the Fall of France and Battle of Britain. He is widely viewed as having conducted the battle in the summer and early Autumn of 1940 brilliantly, by husbanding scarce resources of men and aircraft. Yet within a few weeks of the high point of the fighting in September he was asked to clear his desk without so much as a thank you.

³¹ Watson-Watt (1957), p83.

vacant beat, or have done one of a hundred things that should be avoided in a crucial demonstration...³²

Watson-Watt made explicit here many things that would normally go unremarked when reconstructing an experiment for publication. Such difficulties were the sort of thing that Watson-Watt could, some 22 years later, elaborate upon. At the time of the experiment they were factors that lay in the domain of tacit knowledge, as defined by Polanyi and refined by Collins.³³ As the experiment worked in this case, they did not have to go through a process of learning why their equipment failed, how to make it work, and how to modify it, which they did later with other apparatus. This is the learning process whereby scientists interact with their equipment to gain the embodied skill of how to make it work. Gooding relates how Faraday went through a similar learning process whilst working on the phenomena of electricity and magnetism in 1822.³⁴ The quote also illustrates the fundamentally uncertain nature of any experiment, even when the techniques and equipment are already well known, and the phenomena had been produced before (although not to their knowledge).

Despite the possibility of difficulties, the demonstration was successful in its aim, which was to persuade Dowding and the Air Ministry to allocate money to the project. On the strength of what they saw, they recommended the allocation of £12,300 for the first year, excluding the cost of aircraft flying time. It is interesting to note how much time and effort had to be spent on the demonstration, considering such an effect had already been observed several times by others. However, in these instances it was merely an annoyance to the experimenter, and so its utility to the military was not perceived. But, as Gunston put it in 1976:

When one reflects on the situation the mind boggles as to how it could have come about. The need for a method of unfailingly detecting, locating, and tracking enemy aircraft had been clear and obvious from 1915 onwards. There surely appeared to be only a very limited range of possible methods, each derived from heat, light, sound, magnetism or

³² Watson-Watt (1957), p109. This is Watson-Watt's reconstructed recollection of a conversation he had with Dowding.

³³ See Polanyi (1958), Collins (1985), Ch3.

³⁴ See Gooding (1990), Ch 6.

electricity (accepting further that heat, light, magnetism and electricity are all manifestations of one of the most fundamental phenomena in the universe, the electromagnetic wave). Any bright schoolboy asked to think about the matter would have gone to see eminent physicists, radio and acoustic engineers, and the matter would have snowballed as each worker named others prominent in the same field. Within 24 hours our schoolboy would have heard of Hertz, and of the shoal of subsequent workers who had demonstrated the reflective properties of radio waves and patented Telemobiloscopes. He would hardly have been able to avoid discovering that as lately as 1932 four engineers researching VHF (very high frequency) radio for the Post Office had included in their large published report how cross they had been at interference from aircraft, describing the measured aircraft ranges and the way the beat-interference varied with aircraft speed. In 1933 Bell Telephone Labs in the United States published even more extensive observations. Yet here was Rowe [in 1934] still unaware of any of these things.³⁵

It is, of course, easy to look back with hindsight, as Gunston has done, and imply that such things were there for the finding if one were only to look in the right place. Yet in 1934 the solution was far from obvious. There were several different avenues that were tried before reflection of radio was settled upon. I will show later in this chapter that, even though this method had considerable political and scientific support (from, amongst others, the Tizard Committee, Watson-Watt, and the research establishment dedicated to investigating related phenomena that was formed at their instigation) it was still a far from accepted technique within all circles of power and influence even as late as 1939.

2.3 Chain Home

After the Air Ministry approved funds for the investigation, Watson-Watt organised the commencement of research at his laboratory, the Radio Research Station at Slough. Simultaneously he began to search for a suitable place in which to carry on this secret work in greater privacy than was afforded at the Station. He assigned Wilkins and Bainbridge-Bell, another ionospheric worker, to the design of a receiver. After advertising for further

³⁵ Gunston (1976), pp38-9.

workers, he employed Dr E.G.Bowen to join them. Bowen was already working at Slough, having taken his PhD in London under Appleton (the pioneering ionospheric worker).

Orfordness, on the Suffolk coast, was the location selected by Tizard to house the pioneer radar group. It was an old airfield located on a remote spit of land accessible only by boat, so that it was virtually an island. The airfield, built during the First World War, was originally used as an armaments research station, and therefore had suitable buildings to house the team. Watson-Watt and Wimperis went out to look at the Station in early March, and reported its suitability to the Tizard Committee. It was sufficiently isolated for them to conduct sensitive work away from general view. Initially the group were short of the necessary funds to buy equipment, so they began building the transmitter and receiver using spares from the ionospheric equipment at Slough. The group were to move out there as soon as an electricity cable was laid from the mainland.³⁶

The cover story thought up for Orfordness was that it was an ionospheric research station. To support this story Watson-Watt suggested continuing research on 50m. He believed that better results would probably be achieved by working at wavelengths of 7m, but that their 50m work should be continued for the time being. Components and techniques were already well developed at this wavelength, and this would enable them to gain valuable experience in tracking aircraft before having to get to grips with the new shorter wavelengths.³⁷ By basing their initial research on tried and tested techniques and equipment, the group were saving themselves a long period of getting to acquiring the skills necessary to build and operate new equipment as well as learning how to use it in a novel way. The difficulty of getting newly constructed equipment to work even when the way it should respond was already established would have been very time-consuming, and time was something the researchers felt they didn't have. In this case there would be plenty of problems to come in operating equipment with new goals in mind: those of learning to "see" aircraft with it.

³⁶ Orfordness was frequently referred to as "The Island" by those who worked there. Guerlac (1988), p131.

³⁷ Guerlac (1988), p134. From an interview with Watson-Watt.

2.3.1 Orfordness

On May 13th 1935 the small group of Bainbridge-Bell, Wilkins, Bowen and J.E.Airey (the group's technician) moved from Slough to their new location. Rowe described the "Island" as "One of the loveliest places on earth... I env[ied] the first radar workers who had a fascinating job to do among pink thrift and yellow shingle and the cries of the terns."³⁸ Bowen was not so enamoured of his first view of it: "We were greeted by hail and sleet and a howling East-coast gale... and it was some time before the impression that we had arrived in an Arctic waste faded away."³⁹

They brought two ten-ton trucks full of equipment, and had to transfer them and their loads across the estuary over the next few days. The antenna was erected between two 75 foot wooden towers, and linked to a broad-band receiver built by Wilkins and Bainbridge-Bell. It fed into a cathode-ray oscilloscope indicator, that could give a range-scale of up to fifty miles.

The transmitter was built by Bowen, and was "an unknown quantity [that] had never previously been put together, not even for a test run at Slough."⁴⁰ Their first major problem was generating sufficiently high-powered waves for their transmitter, and they had to "acquire" the necessary components from various sources. The valves were from the Navy Signal School⁴¹, who advised Bowen not to run them beyond their specified ratings. In the interests of rapid progress this was not strictly adhered to. Bowen also recalled to Guerlac that he made a capacitor for his apparatus by fitting together two metal cans, one outside the other. This ability to construct equipment out of the most unlikely sources was to be a characteristic of British radar research (see also chapter 4). The ethos which operated in the British Government research centres was one of "get it working and worry about how it will be engineered later." This approach had both benefits and disadvantages; equipment could

³⁸ Rowe (1948), p13.

³⁹ Bowen (1987), p11.

⁴⁰ Bowen (1987), p12.

⁴¹ Bowen (1987), Guerlac (1988, from interviews with Bowen) and Callick (1990) all give these valves as being type NT46, a silica-envelope valve. Foley (1991), in his history of the silica valve, gives them as type NT41. In any case, the Air Ministry Radar Group were reliant on the Navy's Signal School for high-power transmitter valves during the late 1930's, the development of these being fully covered in Foley (1991).

be made to work very quickly, but on the other hand when in operational service it often proved likely to be unreliable (H₂S was a prime example of this, see chapter 5). Companies such as GEC and EMI were more thorough in their approach to designing new apparatus, but the down side of this was that they were often also less innovative.

This first equipment utilised familiar techniques and equipment, but its performance had to be significantly improved above that required for ionospheric work. Guerlac⁴² summarises the targets that Watson-Watt's team were aiming for:

	<i>Old</i>	<i>Watson-Watt</i>
<i>Peak Power</i>	2kW	100kW
<i>Frequency</i>	6MHz (50m)	less than 6MHz
<i>Receiver Bandwidth</i>	10kHz	50kHz
<i>Pulse repetition rate</i>	-	50/sec
<i>Pulse length</i>	100-200msec	10msec

In order to ensure a return at the ranges required, the gain of the equipment (power out to power received) need to be of the order of 10^9 . The receiver was about as sensitive as it could be, so most of the gain had to be wrung out of improvements in the performance of the transmitter. This involved Bowen running the valves at (short duration) pulse powers well above those that were used in continuous operation. Fortunately the valves stood up to this rough treatment, and on May 31st they received echoes from the ionosphere. This was a useful way for them to calibrate their equipment, by using a well-known phenomenon. These, and later results were published in a paper in 1937.

Just a month later, the CSSAD (Tizard Committee) came to Orfordness to review the progress made so far. Just before they arrived, the team had followed a Singapore aircraft out and back for 15 miles. However, according to Bowen:

Watson-Watt, in his optimistic fashion, had arranged for a Vickers Valentia to fly short legs between Martlesham and Orfordness during the Committee's visit. No clearly

⁴² Guerlac (1988), p136.

recognisable signals were received from the *Valentia*. Many years later Watson Watt was inclined to say that he saw aircraft echoes on that occasion but I am afraid that this was not the case; or at least, if he saw such signals, no one else did. What the Tizard Committee did see was a spectacular display of ionospheric echoes at 60 miles and beyond, but no aircraft echoes. They appeared well satisfied with this.⁴³

Bowen's description acknowledges the early difficulties of deciding what exactly the trace on the screen corresponded to. The team were familiar with the type of echo received from the ionosphere, as they had all come from doing this work. They were also certain of the existence of the aircraft, but at this stage tying down what was the aircraft and what was something else (such as an ionospheric echo) was a difficult process. Watson-Watt did mention the difficulties with this particular demonstration in his autobiographical account, although did not admit them in such a forthright way as Bowen did in his:

When, however, we sent a *Valencia* out to be tracked before their very eyes, we got only intermittent glimpses between 8 and 18 miles - later comparison with the aircraft log indicated that these were right within half a mile - but we failed to "see" him at all on his homeward trip.⁴⁴

On the 17th June, after the committee returned home, the experimenters got much clearer indications that they could track aircraft after getting an echo at a range of 17 miles. It was a *Scapa* flying boat from the Felixstowe base, and when it returned Watson-Watt phoned up the commander to get it to do another trip, which was again observed. They were now in the position of tying down a "blip" on the screen to an aeroplane; in other words they had achieved a calibration and agreed that the signal related to something else, the passage of an aeroplane.⁴⁵ They had now acquired the necessary manipulative and interpretative skills to relate the trace on their screen to a recognisable object, as I discussed in the Introduction. From this point onwards they were able to utilise aircraft from the

⁴³ Bowen (1987), p15.

⁴⁴ Watson-Watt (1957), p127.

⁴⁵ See for example Passveer (1993), on the problems associated with learning to "see" x-ray pictures as a similar problem.

nearby Martlesham Heath base for calibration and testing flights, and with a ready supply of controllable targets the progress they made was extraordinarily rapid (see over for an illustration of what a display looked like).⁴⁶

2.3.2 Difficulties: Personalities and Politics

From the way I have written the chronological order of events it would seem that the progress of the team towards achieving their aim was relatively smooth. They had clearly defined goals, political and financial support, and a team of skilled men familiar with both the techniques and the equipment they were using. This was not always the case, and very often there were difficulties to be dealt with that stretched beyond the business of deciding which particular “spikes” on the CRT came from aircraft and which didn’t.

The radar team on the Island, for the year it spent at Orfordness, was always small in number. It never exceeded a total of six. It was important that the team all shared the same degree of belief in the successful outcome of what they were doing, yet according to Bowen, Bainbridge-Bell had doubts about the viability of the project from the beginning, and was eventually moved back to Slough:

He was undoubtedly the most talented circuit designer at Slough [but] at the time of the move to Orfordness [he] became... cynical... and was very pessimistic about the success of the venture. He saw difficulties which simply did not exist... [H]e was quite certain that the increase in transmitter power would lead to unacceptable overload of the receiver... Almost as his price for joining the project, he... insisted on a very expensive... cable being laid between the transmitting and receiving huts... In the end, [this] problem simply did not arise.⁴⁷

Bowen was making judgement from a position 50 years removed from events. He was able to pass judgement on what could be classed as “difficulties that simply did not exist” because

⁴⁶ Bowen (1987), p15.

⁴⁷ Bowen (1987), p14.

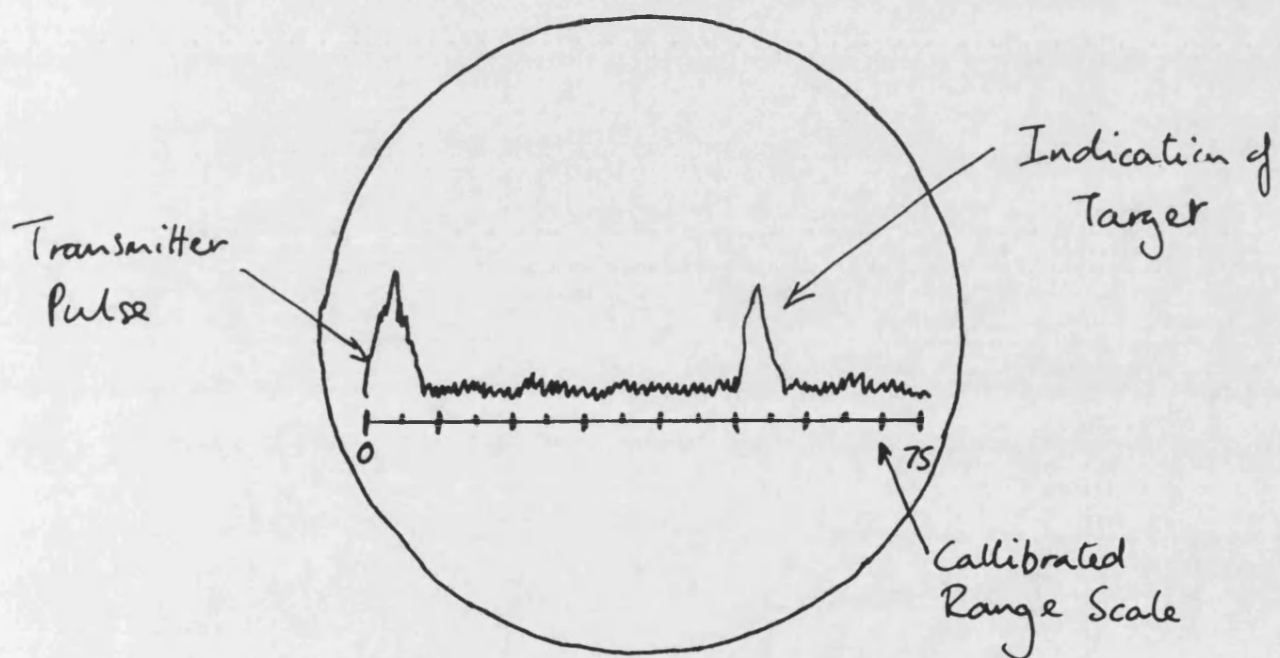


Figure 2.2: The type of indications used to display the presence of aircraft. This method was the preferred method until the all-round Plan Position Indicator was invented. Also known as A-Scope display.

he was aware of what the successful outcome was. Bainbridge-Bell was legitimately casting doubt on whether anything would be achieved, doubt that was probably the expression of a more cautious personality and a realisation that science is never straightforward. At the time his opinion was as justified as Bowen's but their superiors wanted a positive attitude from their staff and Bainbridge-Bell was removed.

In the political arena too, not everyone was completely convinced of the efficacy of placing so much faith in the embryonic radar chain. All the members of the Tizard Committee (the CSSAD) were totally committed to radar, and their visit to Orfordness confirmed in their eyes the necessity for pressing on with the programme with as much haste as possible. However, at the meeting of the CSSAD following their visit an extra person was present. This interloper was Professor Lindemann. Tizard and Lindemann had been very friendly in their younger days, but events of the 1930's and 40's led to a lasting enmity. Lindemann was very strong minded and not afraid to voice his opinions. This made him a controversial figure, and most mentions of him by authors who were involved in events indicate that he engendered in those he dealt with a strong personal bias either for or against him. His involvement with the CSSAD was no exception.

The story of the happenings between Tizard and Lindemann make an interesting adjunct to the main theme. The full story has been chronicled by C.P.Snow, in his study of the workings of the scientific community, *Science and Government*.⁴⁸ Lindemann, as I noted earlier, was Churchill's scientific advisor and linked to right-wing "hawkish" thinking at the height of the appeasement policy. At the time of the 1934 Air Exercises, Lindemann wrote to The Times calling for a scientific investigation of methods of air defence against bombers other than simply by reprisal bombing. This was thought to be the only "defence" against bombing, and operated by the same logic as the later nuclear MAD (mutually Assured Destruction) "balance of terror" argument. Lindemann continued lobbying Parliament for something to be done through 1934 by using his association with Churchill. Importantly, he pressed for a committee independent of any department, such as the Air Ministry. When he was eventually told of the CSSAD in early 1935, and invited to join, he delayed doing so due to his belief that an Air Ministry-linked committee would be bound to be ineffective, and so

⁴⁸ Snow (1961).

not worth his while. Not least behind Lindemann's motives were his politics: the government of the day was led by a Socialist, and he was a staunch Conservative.

Lindemann's strategy led him to agitate for a sub-committee of the Committee for Imperial Defence to be formed. He wanted it to contain a few scientists and service-men, and be chaired by a person of Cabinet rank or equivalent, in order to assure the necessary political "clout" to prevent what he perceived as Civil Service inertia from obstructing their decisions. This committee, chaired by Lord Swinton, was eventually formed in April 1935 and reported to a higher strata of government than the CSSAD. However, Tizard was also made a member of this new committee, and saw a high probability of conflict arising where the responsibilities of the two committees overlapped.

Following the change of premiership between MacDonald and Baldwin on 7th June, Churchill was invited onto Swinton's committee. He made Lindemann's inclusion on the Tizard Committee a condition of acceptance. This was the reason why Lindemann attended the tenth meeting of the CSSAD on July 25th.

Clarke⁴⁹, Tizard's biographer, identifies two main areas where the two men differed. Firstly they had differing opinions on the technical solution to the air defence problem. Tizard and the CSSAD believed that the bomber threat *could* be met by directing sufficient aircraft onto incoming raids, via an integrated radar warning system. Lindemann believed (rightly, as it turned out⁵⁰) that radar stood a great possibility of being jammed and hence rendered ineffective. Therefore fighter aircraft would *not* be able to meet the bombing threat. He believed instead that the correct approach was to sow fields of aerial mines attached to parachutes. Tizard was against this for two reasons. Firstly, he believed that Lindemann's idea for aerial mines was completely impractical. Secondly, "radar had grown up under [his] own loving eyes, and it was natural enough that [he] should tend to deal with such criticisms more leniently than did Lindemann."⁵¹ Lindemann was rightly critical of

⁴⁹ Clarke (1965), pp125-7

⁵⁰ During the Second World War both sides conducted experiments into jamming radar by blocking transmissions and by the dropping of aluminium foil "tuned strips" to blot out responses. This latter was codenamed "Window" in the UK, and "Duppel" in Germany. Each side was initially amazed to learn that the other possessed radar, and neither side utilised effective jamming until much later in the war. This was largely due to an earlier reluctance to provoke the enemy into retaliatory jamming.

⁵¹ Clarke (1965), p133.

radar, but the perceived impracticability of his alternative solution did not do him any favours in winning his argument.

The other major difference between the two, according to Clarke, was their approach to getting things done. Lindemann believed that the ends justified the means, and would use whatever means he had at his disposal. He often made enemies in the process. By contrast, Tizard's style was much less confrontational. Matters between the two came to a head a year later when the whole CSSAD resigned in protest at Lindemann's continued presence. It was reformed without him. Lindemann finally gained the upper hand in 1940 when Churchill became Prime-Minister, and appointed him as his Scientific Advisor. Lindemann made sure Tizard was effectively sidelined, but by then the radar chain was in position and about to prove its worth in the Battle of Britain.

A further example of the reactions that Lindemann provoked can be seen from these quotes from Bowen, writing in 1987. The discussion in the first quote took place during the June visit to Orfordness of the CSSAD:

I heard mention of Professor Lindemann, the controversial Professor of Physics at Oxford University. At that time both Oxford and Cambridge University were represented by members in [sic] Parliament. Professor A V Hill, already on the Tizard Committee, was the member for Cambridge. Lindemann had just put his name forward as a Conservative candidate for the Oxford Seat, but his candidature was turned down by the Party. It may be hindsight, but I seem to remember a distinct feeling of satisfaction being expressed at the time about this set-back for Lindemann. Had we but known, there were to be ominous developments later.⁵²

[I]n January 1935... [t]here were... any number of suggestions about air defence ranging from those which were barely possible to others which were slightly eccentric to still more which were totally irrational or else downright fraudulent. People like Professor Lindemann of Oxford University, for example, advocated wires trailing from aircraft or drifting balloons. The kindest thing one can say about most of these schemes is that in spite of much discussion and many expensive trials, they never lead to a workable method of defence.⁵³

⁵² Bowen (1987), p15.

⁵³ Bowen (1987), p4.

Despite the occasional visit such as those described above, the researchers at Orfordness remained largely unaware of the arguments and intrigues in the corridors and committee rooms of Whitehall. During the remainder of 1935 work progressed steadily on fulfilling the requirements of Watson-Watt's memorandum. The main goals were developing facilities for measuring range, bearing and height. With the co-operation of pilots and aircraft from the nearby Martlesham Heath airfield, they improved the performance of their transmitter by using calibration flights, to the point where in December 1935 they could follow aircraft to a range of 80 miles. In July 1935 they first noticed that a formation of three aircraft produced a fluctuating CRT trace; in other words the spot "wobbled". This effect became extremely useful as it allowed skilled⁵⁴ operators to estimate the size of formations, allowing effective allocation of aircraft resources to meet incoming raids. The problem of height-finding was solved by Wilkins, who used a method he had devised for measuring the angle of incoming transatlantic signals.

The type of direction finding facility was finally decided upon around September, when a crossed-dipole method was tried and found successful. A previous suggestion to use two receivers and work out bearing from triangulation was rejected as being too complicated for the operator. During this period they reduced the wavelength of the transmitter from the original 50m (inherited from ionospheric research) to 26m, due to interference they received from 50m signals from transmitters on the Continent. This was again reduced to 13m due to interference from domestic 24m transmissions.⁵⁵

2.3.3 Bawdsey

On 9th September 1935 Watson-Watt submitted a progress report to the Swinton Committee (the Sub-Committee of the Committee for Imperial Defence) summarising the state of research at Orfordness. Part of this document was a summary of discussions he had

⁵⁴ By this, I mean operators with the necessary interpretative skills. In other words, they would be operators who could interpret the images on the screen - a skill in itself - but one different from having a more interactive skill of being able to modify the equipment to produce the required effect as well as interpret it.

⁵⁵ Swords (1986), pp186-95.

held on the nature of the warning system that would need to be introduced in conjunction with the radars for them to be effective. They recommended building a chain of 20 stations around the South and East Coasts. The Swinton Committee accepted these recommendations, and voted further funds for the setting up of this chain and the expansion of the research establishment dedicated to radar.

Towards the end of 1935 Watson-Watt realised that the limited facilities at the Orfordness site were no longer suitable for housing the establishment, as it grew in size. The prototype early-warning station required the building of a 240 foot mast. This was in close proximity to an airfield where it was a hazard to flying. Also, the staff numbers were rising, and laboratory space was running out. Watson-Watt and Rowe went to search for larger premises, and settled upon Bawdsey Manor, a large country house located 15 miles further down the coast than Orfordness. The house was owned by Sir Cuthbert Quilter, and had been built by his family in the latter part of the previous century in a mixture of Indian styles. The Air Ministry arranged a compulsory purchase and the team moved there in May 1936. Bawdsey was almost unique in East Anglia in being sited on a raised piece of ground, some 80 feet above sea-level. This fact meant that an extra 80 foot of height could be gained for the radar towers without any extra building. The manor house and associated outbuildings provided accommodation for the experimenters, and adequate laboratory space.

In the Summer of 1936 Watson-Watt resigned his position at Slough and formally took over as Superintendent of the Bawdsey Research Station. He recruited many staff from the Universities and Technical Institutes, and at first the atmosphere was more that of a University than of a Civil Service Establishment. Hours were not regular, work continuing late into the night whilst croquet and cricket were played during the day.⁵⁶

Despite the urgency of the project things did not always progress quickly due to a significant culture clash between those who hailed from educational establishments and those whose background was in the Civil Service. Ex-University men, used to popping down to stores or even going out to purchase their own equipment, found it difficult to get to grips with an Air Ministry stores procedure that seemed to operate at a pace more akin to that of a snail than a top-secret, highly urgent military project. No component could be ordered

⁵⁶ Robinson (1983), p27.

without forms in triplicate, for example. Another major point was that, contrary to what one might expect from an establishment using radio techniques and components, very few of the staff were radio engineers. As Robert Hanbury-Brown writing in 1991 pointed out:

What was... remarkable about our group... was that there was not a single experienced radio engineer. In the receiver group our boss had some knowledge of radio, but I wouldn't have described him as a radio engineer, while Donald Preist and myself were both straight out of college. Admittedly I had attended a formal course in electrical engineering and my head was full of the mathematical theory of things like filters and antennas, but that was of limited use... what we needed was practical experience. Fortunately both Donald and I had been keen radio amateurs and had built our own equipment; without that experience we would have been lost.⁵⁷

This tendency to employ people in radar research who were not radio specialists is a recurring theme in this history. The team who developed centimetre components, and then centimetre radar, were very much in the same mould. It may at first glance appear strange, but usually the non-specialists had skills that could be useful, or were radio amateurs. Certainly Hanbury-Brown was well equipped in other areas for his work; as he informs us he had long been used to tinkering with radio components and so had a feel for using them. It was, post-hoc at least, a definite policy to employ such skilled non-specialists. The heads of the various teams and establishments felt that people who were not steeped in the culture of what could or could not be done, were much more likely to follow unusual and original paths. They would, therefore, be quicker to gain solutions to the pressing radar problems than those who were part of the established radio industry. Watson-Watt deliberately employed non-radio engineers in order to "improve speed and originality"⁵⁸.

Some historians⁵⁹ have argued that this simply caused delays at later stages of the process of manufacture. I am inclined to agree. When one reads of the German equipment

⁵⁷ Hanbury-Brown (1991), pp12-3. Hanbury-Brown was in the unusual position of having gained a degree by the age of 19. This was because, having had to leave public school when his step-father absconded "in a cloud of debt", he continued his education at Brighton Technical College. Whilst beginning his PhD at Imperial College he was "head-hunted" for the radar project by Tizard.

⁵⁸ Guerlac (1988), p144.

⁵⁹ See for example, Süsskind (1968), p102.

captured periodically by the British there was always great praise for the quality of its manufacture.⁶⁰ German equipment was developed by commercial firms, and not in Government establishments. On the one hand this led to the superb build-quality mentioned. On the other hand, the Germans often lacked a clear idea of the purpose for the radar equipment they designed (see chapters 6 and 7). British solutions were often innovative, but being rushed into operational use often had poor serviceability. The first operational use of H₂S which was rushed into service, as chronicled in Chapter 5, is a case in point. Attempts were made to rectify this problem during the war by the introduction of what became known as "Operational Research", a process that was refined by Sir John Cockcroft, a member of the Cavendish Laboratory and a colleague of many members of the centimetre radar development team.⁶¹

The Swinton Committee decided upon the locations of the first seven (later reduced to five) radar stations in the coastal chain at a meeting in the September of the previous year (1935), and issued instructions to commence their construction. They originally intended that these stations be ready in time for the 1936 Annual Air Exercises, in September. Unfortunately construction did not proceed to schedule, and only the prototype research station at Bawdsey (which was, however, to become part of the chain in any case) was ready to participate in the exercise.

Despite these setbacks, the Air Exercises took place over three days and involved around a hundred aircraft. Half the aircraft were bombers detailed to "attack" Bawdsey by approaching from 100 miles out to sea. The other half were fighters that were to be directed onto the raids by operators using the radar at Bawdsey. The Commander-in-Chief of Fighter Command, Sir Hugh Dowding, who had recently been promoted to this position from that of Air Member for Research and Development, was invited to observe the exercise.⁶²

It was vitally important for the continuation of funding for the project that this demonstration was successful, as it had already been delayed several times due to the

⁶⁰ Nissen (1989).

⁶¹ Operational Research was a phrase first coined by Rowe in 1938. It was applied to the work done by a team investigating how to improve the process of filtering the information received at the control centre from the stations. It was later extended to the whole process of investigating how new devices fitted in to their place of operation. For a full history, see Air Publication 3368 (1963).

⁶² Bowen (1987), p24.

construction of the other stations being behind schedule. Unfortunately, the first day of the Exercise was a disaster. No signals were received from incoming aircraft in time to make interceptions, and Dowding "sat in the receiver room and saw nothing until his ears told him that the raiding aircraft were passing overhead."⁶³

At the time of this exercise Bowen was no longer connected directly with the early warning radar; he was now in charge of building an airborne radar (see section 2.4). He had already built a miniature receiver, and a ground-based transmitter in one of the towers at Bawdsey Manor and on the day of the exercise was able to view aircraft echoes to a range of 50 miles using this much more experimental and less powerful equipment. At lunchtime on the first day he was informed of the problems with the main equipment, which were that the demonstrators had only managed to obtain a range of 10-15 miles from the main transmitter, and that as a consequence no interceptions were made.

Bowen described what he could see on his own equipment to Watson-Watt, who immediately arranged to show Dowding. This improved matters somewhat, in that at least something somewhere was visible. However, that afternoon the main transmitter, which was after all the reason for the demonstration, could not be made to work. That evening a post-mortem was held, and Bowen's description of the discussions is interesting:

A very confused picture emerged from the subsequent discussion. Some put the blame on the new transmitter on the 240 foot tower on the hill, but there were others who defended it. The issue should have been clear cut, but there was a complicating factor. Wilkins had recently replaced the open wire feeders on the receiver towers with coaxial cable and there had not been sufficient time for proper tests to be made.⁶⁴

In this particular experiment, there was not a replication of the original apparatus as some of the equipment had been modified. Several of the variables in what was previously a working system were changed, and as a consequence the system no longer worked. The experimenters were certain about what the outcome of their experiment should be, but they could not make that correct outcome happen. They needed to replace different pieces of the

⁶³ Bowen (1987), p24.

⁶⁴ Bowen (1987), p25.

apparatus to establish which one was the “culprit”.⁶⁵ What was required was someone with the requisite knowledge to recognise what elements of the equipment were crucial to operation and shouldn’t be replaced. Bowen was that person, as he had helped to construct the apparatus, and had the embodied skill (or “know-how”, following Ryle) of both the articulable and unarticulable kind necessary to effect modifications.

Upon investigation, Bowen found that there was virtually no signal being transmitted from the aerial tower. He was dispatched to Orfordness to resurrect the transmitter used previously before the new one at Bawdsey was constructed. This one was fully understood by the operators who knew how to get it to produce the correct result, and was far more suitable for demonstration purposes. Bowen worked overnight to repair this transmitter, which gave a satisfactory performance the next day and mollified Dowding.

Hanbury-Brown was also working at Bawdsey that day, but contrary to the earlier comments in his autobiography, he put the blame for the mistakes they made on them not having enough experienced engineers:

The almost complete failure of the radar to perform on the first day was a good example of the need for more engineering *know-how* [my italics]. The man whom Watson-Watt had put in charge of the transmitter, although very bright, was a physicist with no experience of radio at all. He learned his radio engineering on the job, and by studying my copy of *Short Wave Wireless Communication* by Ladner and Stoner. I think we all began to fear for the future of that transmitter when, at tea one day, he turned to me and said ‘Hanbury, how can I break down the sharpness of resonance?’ You don’t have to know much about radio engineering to realise he was starting from scratch!⁶⁶

There appears to be a difference of opinion here about whether it was better to have specialists or non-specialists working on radar. In this case, they were working on new applications of existing technology, so Hanbury-Brown was probably right to believe that people with more “engineering know-how” were required. Later, as I relate in chapters 3 and 4, the new innovations were performed by people with experimental know-how in terms

⁶⁵ Compare this with Collins’ account of replicating a laser, where the original laser could not be successfully replicated by instruction alone. Collins (1985), Ch3.

⁶⁶ Hanbury-Brown (1991), p16.

of building apparatus, but without radio-engineering training (and the associated preconceptions) about what was and wasn't possible.

Despite the success of the Bawdsey transmitter in allowing operators to locate the position of incoming aircraft, positional information was no use on its own. For radar to fulfil its intended role of air defence, that information had to be used to direct fighters against bombers. This meant transmitting the positional information of incoming aircraft to Fighter Command Headquarters. Bainbridge-Bell devised a method of transmitting it so that it was displayed as a spot on a CRT map, indicating exactly the position of the aircraft. Designed to save the operator from having to work this information out herself⁶⁷, it failed to work this way *in practice*:

[I]t drove us nearly mad and might well have wrecked the whole operation; the data supplied by the radar was dubious enough to start with, without the unknown errors which were introduced by this gadget when one of its many cords slipped. We gave up using it on the second day of the Exercise and took to calling the results down the telephone.⁶⁸

Earlier on, in July, Tizard pressed for an investigation into how the information provided by the radar stations would best be utilised. This led to a series of experiments at Biggin Hill airfield, whereby fighters were sent up to intercept incoming aircraft. After a few weeks (the experiment began on August 4th) the controllers became competent at direction, even when the incoming aircraft changed their course during the interception. In practice, it was found that often the angles of courses to be transmitted could be *estimated* with sufficient precision to make an interception, negating the need for calculating the triangulation of vectors exactly and thus saving time. The operators became very skilled at using their new apparatus. The difficulty with this new style of fighter control was selling it to the pilots. Traditionally the role of the fighter pilot was to wander the sky at his own volition, and many of them did not take to the idea of being little more than pawns in a game of 3-dimensional aerial chess. With the new system, their individuality was subordinated to

⁶⁷ Many radar operators in the Chain Home Stations were women, as they were found to have a better concentration-span than their male colleagues.

⁶⁸ Hanbury-Brown (1991), p14.

orders from older men on the ground, directing them by means of a mysterious, secret "magic eye". That radar *was* successfully brought into operation, in light of these possible obstacles, was no mean achievement.⁶⁹

Just as these experiments in aircraft direction began to show promising results, Tizard was made aware of the disastrous Bawdsey experiments in mid-September by Dowding. In the light of the poor showing he wrote to Watson-Watt saying that due to political pressure he would soon be forced to recommend that the programme be curtailed, unless they could make improvements in time for a repeat test in April 1937. Experiments on interception continued during this period, and fortunately the next time they chose to demonstrate it to the "Top Brass" they conducted a much more convincing experiment. Construction of the individual stations in the Radar Chain continued over the next two years.

2.3.4 Operational Use

Between 1936 and 1939 the original solitary experimental Bawdsey transmitter and receiver station expanded into a full twenty-station early-warning chain. The first five stations were in place by the time of the Munich Crisis in 1938, and because of the perceived German threat were put onto 24-hour watch. These first five stations surrounded the Thames Estuary, as London was thought of as the main target for any attack, the five would stand in the way. The chain as originally intended extended all along the South and East Coasts. All the positional information from the stations was telephoned into a central filter-room, which was in turn connected to the fighter airfields so that they could be told to take off and intercept. Further experiments were conducted that exposed flaws in the integrated system, so that by the time war broke out in September 1939 it had been refined pretty thoroughly. Actual experience during the next year meant that the system worked effectively during the Battle of Britain, in conjunction with other innovations such as the

⁶⁹ Clarke (1965), pp150-3. Some pilots did choose to ignore the controllers, but on the whole most obeyed them when the system was put to the test in 1940.

aircraft themselves,⁷⁰ Identification Friend or Foe⁷¹ and the VHF radio-telephony links (the latter two arising directly from experience of using the integrated system).⁷²

The first five stations were still prototypes, and flaws discovered during the exercises in 1938 were eradicated from the others.⁷³ The serviceability rates of the first five were not as good as the others, which lends weight to Hanbury-Brown's view that if more engineers had been involved earlier on, there would have been more reliable stations from the start.

A further problem that was encountered with the Chain Home (CH) stations, as they became known, was that it was very difficult to pick up aircraft at very low heights. This was due to the long wavelength and floodlighting principle that they worked on. To compensate for this a range of sets were developed from the prototype 1.5m airborne set to fill in the low-coverage gap. Known as Chain Home Low (CHL) it employed a rotating aerial, and was introduced in October 1939.⁷⁴

2.4 The First Airborne Radar

In this section I shall describe the development of the first airborne radar. The happenings in this section are important for several reasons. Firstly the members of the airborne radar team gained much experience in developing radar devices to fit in aircraft. In particular, they were used to working inside aircraft in the air, and operating their equipment in the more difficult conditions found there as compared to the laboratory. Although the

⁷⁰ The Hurricane and Spitfire were both designed in 1935 and entered service in 1939. They were the first monoplane fighters in service with the RAF and represented a generation leap in performance over the previous biplane fighters.

⁷¹ IFF was developed to allow recognition of friendly aircraft on the Chain radar screens. When illuminated by them, the IFF box carried by a friendly aircraft transmitted a pulse on a specific frequency that appeared as a characteristic spike on the trace of that aircraft. Not all British aircraft were fitted with it by the Battle of Britain, but it was later to become standard.

⁷² To enable efficient communication between pilots in the air and controllers on the ground, and for pilots to talk to each other in the air, VHF radio-telephones that were frequency modulated and hence much clearer than the old audio modulated sets were also developed in time for the Battle of Britain.

⁷³ Technical details of the CH stations can be found in Swords (1986) Ch5, and Neale (1985).

⁷⁴ Jones (1985), p417.

head of the team, Bowen, was marginalised as far as the main protagonists were concerned by the time centimetres came along, he was still able to pass on some of his experience to his successors (see chapter 4). Secondly, the airborne team had many opportunities to provide solutions to how to actually design and build an airborne radar. I shall follow some of the processes that they went through when there were no fixed conventions about how to do things. By so doing I hope to make visible the way that science is creative in the way it works.

The airborne radar team, like the Chain Home team that preceded it, and many teams to follow (see chapters 3 and 4) was small, close knit, shared common work practices and beliefs, and had practical skill in working with electronic apparatus. I wish to examine the process of the transfer of embodied knowledge between individuals within a group given these conditions, and see how they fit with my analysis of these points in the Introduction.

2.4.1 The Need for Airborne Radar

The decision to attempt to build what seemed to be an impossible device was typical of the British approach to radar of the time. The reasoning behind the decision to try to develop an airborne set hinged on the early success of the radar team. It was thought that the new device would mean that the defending fighters would be able to successfully counter any attacking daylight bomber force. Facing heavy losses would lead, they reasoned, to the Germans switching their attacks to night-time. Although the CH stations and operators could place fighter aircraft to within 3 or 4 miles of incoming bombers, near enough for visual target-acquisition during daylight, visibility at night is only 500-1000 feet. Therefore pilots with no additional aids would not be able to spot attacking aircraft at night, even if placed in their general vicinity. It was thought that an aeroplane-mounted miniature radar set was required to make up for this deficiency.⁷⁵

⁷⁵ Later experience showed that a more accurate radar than chain home was required even for placing night-fighters in range for their own sets to receive signals. From this, Ground Control Interception (GCI) stations were developed, entering service in October 1940. Even with this innovation, which was based on CHL and included the first Plan Position Indicator display to aid the controller, the pilot and his radar operator had to

My use of the conditional in the previous paragraph is deliberate, for who thought of the need for such a device is not exactly clear. It appears to have been either Watson-Watt, or Tizard. Guerlac merely said that "one of the members of the Tizard Committee pointed out the necessity for a small radar set that could be carried in an aircraft."⁷⁶ Gunston was more specific: "While... [CH] went from strength to strength, Watson-Watt - many said Sir Henry Tizard thought of it - lost little time in beginning [airborne radar]."⁷⁷ Rowe came down on the side of Watson-Watt: "Early in the history of radar development, Watson-Watt had forecast that radar sets would one day be installed in aircraft. This was indeed vision of the boldest kind."⁷⁸ Watson-Watt (in 1957) concurred with this:

Bowen has recalled that it was on some date... late in 1935 that I first dared to propose... that we should compress a radar station into an item of aircraft equipment... The project was put by me, as the voice of the team, to the Tizard Committee in February 1936.⁷⁹

I not discovered anything which gives Tizard's own views. However his biographer, Clarke, quotes Bowen as saying "Tizard was in fact the one who in 1936 first suggested that an attempt be made to make a radar set into an aircraft."⁸⁰ Bowen says in his own book that "[Tizard] passed [his] thoughts on to Watson-Watt and Watson-Watt passed them on to us at Orfordness."⁸¹ What we see from all these accounts is another example of how memory is an imperfect recording device, and is prone to exaggerate the part played by an individual when they recollect what they participated in. However, when we are able to compare several accounts, as I have here, we can see that the issue cannot be settled by recourse to what the original actors recall or have written, so it is probably fairer to give equal credit for

learn how to effect interceptions using the new AI equipment. Some were far more successful than others, such as Rawnsley who was operator for "Cats-eyes" Cunningham, the most successful British night-fighter pilot. See Rawnsley & Wright (1957) for descriptions of how the routines were painstakingly evolved over a period of several months of trial and error.

⁷⁶ Guerlac (1988), p145.

⁷⁷ Gunston (1976), p41.

⁷⁸ Rowe (1948), p39.

⁷⁹ Watson-Watt (1957), p142.

⁸⁰ Clarke (1965), p158.

⁸¹ Bowen (1987), p31.

thinking of the idea to Tizard and Watson-Watt. This example does illustrate how it is important to take note of a variety of sources if at all possible for the sake of making sure that it isn't one person's recollections which get sole status as "the truth", as usually there is some variation which gives the historian the scope to make an interpretative judgement about what happened.⁸² Leaving aside the issue of who first thought of it, the perceived need for some form of airborne radar came very early in the history of radar in Britain. This, as I indicate in Chapter 6, was something unique to this country and shows how technology is shaped by the cultural niches in which it is required to fit.⁸³

2.4.2 Airborne Interception (AI)

Bowen was put in charge of this new project in early 1936, and initially he worked without assistance. The scale of the problem was enormous, and must have looked insurmountable. Tizard was asking Bowen to take many tons of equipment, including high towers, buildings, generators, etc., and compress this into an equipment weighing at most a few hundred pounds and occupying a minimum of space. One could almost compare it as going from ENIAC to a PC in one step. To get some idea of how the problem appeared, it is worth extensively quoting Bowen's autobiography, where he clearly set out what the problems were:

No one was very optimistic about being able to make a miniature radar at that time. The existing transmitter at Orfordness was a whole room full of equipment weighing many tons. The transmitting and receiving antennae were on masts 75 feet high, soon to be increased to huge structures 240 feet high. The receiver was a large rack of equipment bristling with valves, control knobs and indicators requiring the attention of a highly skilled operator... There were other formidable obstacles. To achieve a reasonable antenna size, the operating wavelength would have to be reduced to one or two metres. This was a

⁸² See, for example, Gooding's (Gooding (1993)) discussion of Holmes' biography of Krebs, and Appendix A, for further discussion on the role of memory in shaping autobiographical accounts.

⁸³ For discussion of how "perceptual filters shape strategic goals", see Beyerchen (1994), and my discussion in chapter 6.

time when the shortest wavelength for which components were available was five meters. In addition, in order to achieve a minimum range of 1000 feet, the pulsewidth had to be reduced to near one microsecond, that is, to one millionth of a second. The reduction from 200 microseconds to 20 microseconds which had already been achieved at Orfordness had not been difficult, but going down to 1 microsecond was strictly unknown territory.⁸⁴

My first step was to draw up a list of the design criteria which should be aimed at in an airborne system. There was no such thing as an operational requirement and no precedents by which mundane considerations like size and weight could be evaluated... [T]here were as many opinions as to what a night-fighter would look like as there were people prepared to make a guess.⁸⁵

By “what a night-fighter would look like” Bowen is being literal; no-one had fixed how the actual aeroplane should be designed. One engine or two? A single crewman, two, or three? Bowen clearly illustrates in these quotes what he had to consider in relation to the task he had been set. It must have been a challenge for him, and it is also very useful for the historian because he sets out before us the process through which he had to go. There were no right or wrong ways to go about building an airborne radar, it was up to him, in conjunction with the few colleagues who soon joined him, to work out how to solve the problems using the skill and knowledge they had accumulated whilst working with the chain radars, and in their earlier laboratory work. Importantly, it is also very useful for the historian to try and uncover what other options were considered, whether these options were tried and if so what the outcome was that led them to change their minds. When the history of science or technology is written it is very easy to write it as a natural progression along lines that must have been obvious. This is not the case, and Bowen has illustrated it well. A similar view was related by Hanbury-Brown, who again summarised the problems well despite joining the small airborne group a year later in 1937:

In the development of things like... radar there is an early and interesting stage when all the designers' options are still open and the conventional pattern is not yet set. If, for

⁸⁴ Bowen (1987), p31.

⁸⁵ Bowen (1987), p32.

example, you look at the history of radio you will find that before the invention of the thermionic valve, all sorts of unlikely devices were used to generate and detect radio waves... When the diode and triode were eventually invented the pattern was set... In 1937 much the same was true of airborne radar, most of the questions of how to design the actual electronic components and all of the questions of how to make a radar operationally useful in a military aircraft were still wide open; there were many possible answers and that made the work exciting.⁸⁶

Bowen was soon joined by other workers, and despite the rather awesome task ahead of them, the nature of the work was stimulating. During 1936 Bowen made progress in defining the criterion for his embryonic airborne set. He consulted aircraft engineers to find out the sort of constraints within which he had to work. The set would have to be not more than 200 pounds in weight, occupy no more than 8 square feet and consume no more than 500 watts of power. Most of the available power on the aircraft was already used in running existing services such as the instruments and the radio. A group discussion on the effects of drag on performance ruled out wire aerials, so only short stub-antennae could be used, requiring a wavelength of around 1 metre. There were further considerations to take into account, such as making the set easy enough to be operated by either a pilot, or a dedicated operator, in a cramped, noisy, blacked-out cockpit.⁸⁷

One of Bowen's first major breakthroughs was the acquisition of a receiver from EMI, developed for their proposed television service. His earlier attempt to build a receiver on 1.5m was abandoned due to the complete lack of any suitable high-power transmitter valve at this wavelength. The EMI receiver was very sensitive, and suitably small and light for installation in an aircraft.⁸⁸ By the time this receiver was ready, the radar team had moved to Bawdsey, and Bowen's airborne group was expanded to include three new members. One of these, Gerald Touch, had previously studied at the Clarendon laboratory for a PhD. The other two, Hibberd and Jefferson, were from the radio industry.

Experiments literally "got off the ground" when they installed the newly acquired EMI receiver in an aircraft. Bowen decided that the only way to begin was to leave the

⁸⁶ Hanbury-Brown, (1991), p21.

⁸⁷ Bowen (1987), p32.

⁸⁸ Guerlac (1988), p147.

transmitter on the ground. At this stage there was simply no way they could make a transmitter small and light enough for installation in an aircraft. Prior to this step the transmitter and receiver, both working on a wavelength of 6.7m, were first tested on the ground where ranges of 40-50 miles were achieved against aircraft.⁸⁹

The aircraft used for testing was a Heyford bomber. This was one of the typically antiquated aircraft of the RAF of the period. It was a biplane bomber with open cockpits and a fixed undercarriage, but despite these defects it had one advantage very useful for radar in that it had engines that produced a relatively small amount of electrical "noise". The first flights took place in the Autumn of 1936, and produced very good results. At a flying height of 2-3000 feet, ranges of 8-12 miles were achieved. Bowen put these achievements in perspective by pointing out that such ranges were never exceeded, especially at such low heights, by *any* production airborne radar set throughout the duration of the war, including the centimetre ones.

After this trial and initial success using a ground based transmitter which had the advantage of not being constrained by weight and space, and allowed them to use much higher powers than would be available in an aircraft, they now had to make a decision on whether to carry on with this method. They called it RDF 1.5, which indicated that it was a "half-way-house" between the Chain radars (RDF 1), and full airborne radar (RDF 2). Bowen was strongly committed to this solution, despite its difficulties. The main problem was that the range measurement was not accurate unless the aircraft was directly between the target and the transmitter. Unfortunately for Bowen, Watson-Watt was against the idea, and despite continued pressure, it was dropped. Bowen believed that by so doing they lost the opportunity to gain valuable experience in the usage of airborne radar, in much the same way that the Biggin Hill exercises allowed the RAF to iron out their problems with fighter direction. Unfortunately for Bowen, it was also not the last time that he would be over-ruled to the detriment of rapid progress, in his opinion.⁹⁰

At around this time the team acquired some new valves from America, known as "door-knobs" after their bulbous shape. They were ordered after their attempts to modify the

⁸⁹ Bowen (1987), p35.

⁹⁰ Bowen (1987), p37.

existing British-made valves to work at low wavelengths and high powers were unsuccessful. The intention was to use them to push the wavelength of the transmitter down to around 1m. Touch built a transmitter with these valves working on 6.7m in order to try it in the Heyford aircraft with the EMI receiver. Successful operation again came quickly when the complete assembly was tried in March 1937. The cranes at the harbour in Felixstowe showed up particularly well, being large metallic objects. After the rapid progression on the CH stations, they felt sure they could make similar progress with the airborne radar (or AI, for Airborne Interception, as it became known).

Despite at the outset having a clear idea of the applications of the airborne set (AI, and Anti-Surface Vessel [ASV]), there were still a large number of obstacles for them to overcome. Their eventual aim was to produce something that could be used by a specially trained operator. However, the operator would not necessarily have any knowledge of science or electronics, and would be working in cramped, cold, dark, noisy and shaky conditions, trying to find an enemy aircraft whilst simultaneously trying to avoid getting shot down or having an accident himself. It was therefore vitally important to take all these conditions into consideration when deciding how best to design the display of the set. As Hanbury-Brown put it:

[O]ne had to imagine oneself in a night-fighter chasing some bomber in the dark, [or] skimming over the sea looking for enemy ships... What precisely should the radar do? Where could it go in the aircraft and who would operate it? How should the data be displayed?⁹¹

Soon after the flights in the Heyford, Bowen's group were offered their own aircraft. To borrow aircraft from the Experimental Establishment at Martlesham Heath was increasingly difficult, as they were already fully committed to their own programmes of testing and couldn't fly at the whim of the men from Bawdsey. After some thought, they selected Avro Anson, and two were delivered in August 1937. This aircraft was a twin-engined monoplane with a large internal cabin suitable for carrying scientists and their

⁹¹ Hanbury-Brown (1991), p23.

equipment. It was also sufficiently modern to offer a modicum of reality in terms of simulating the conditions of the end use of the radar, in a high-performance fighter aircraft. The two Ansons were specially modified so that their ignition systems were screened, to prevent interference with the received signal. The normal level of ignition noise was high enough to completely swamp the receiver, and therefore had to be eliminated. This was a problem of many aircraft of the time. Later aircraft that were designed especially to carry radar had screened engines from the outset. Others converted to this task of carrying sensitive radar receivers had to have their engines modified in this way.

In the meantime Bowen and Hibberd worked on their transmitter, and Touch on the receiver. Their task was to try to get it to operate on a wavelength of around 1m. It was successfully made to operate at 1.25m, but its performance fell markedly at wavelengths below this level. They calibrated the set on the ground using a water tower 3 miles distant as a target. Such calibration using large and obvious fixed points was a common means of verifying that the set was actually locating something “out there” as oppose to merely displaying “noise” (see also section 1.2.1). This equipment was first tried out in an aircraft on 17th August when Bowen recalls that they received echoes from ships at ranges of 2-3 miles.⁹²

During these first few months of operation, ship detection proved easier than aircraft detection due to the larger size and slower speed of ships. Following further ship observations during fleet exercises held in September, priority was gradually switched from AI to ASV. This decision was largely “political”. originally, of course, the whole rationale for airborne radar was to provide a means to combat bombers. However, when they discovered that one could also detect ships with the new apparatus the focus of the end-use shifted away from the more difficult AI project. Nevertheless, they still conducted their early ASV experiments with crude apparatus that often refused to work at all, and gave only range data. They still had no means of direction finding, and the transmitter was very underpowered.⁹³

⁹² Bowen (1987), p41.

⁹³ Guerlac (1988), p148.

This shift in emphasis continued for a year, but when they recommenced work on the AI programme in the summer of 1938 the development work done on ASV meant that the airborne apparatus available was much improved from the previous year. For example, whilst using the Heyford, power for the equipment came from an assortment of batteries. After switching to the Ansons, changed to a generator. Standardising the power source was very important, for up until this time they had had to contend with different current and voltage ratings, and supplies that varied in rating whilst in use. This could affect the performance of the equipment severely, because it would perform in an unexpected fashion or not at all. The variation in power supply was one more unwanted factor that would have to be investigated when they were looking to fix a problem. Bowen decided that the solution was to run an alternator off the aircraft's engine. He made a trip to electrical manufacturers Metro-Vickers and discussed his requirements with them, asking them to come up with a suitable design. Their design was adopted, and performed two extremely important roles for Bowen. Firstly, it standardised the power supply to their equipment, eliminating one more variable in their experimentation. This had the effect of embedding Bowen's skill into a piece of apparatus. He learned when it was the variable supply that caused problems with his apparatus, but doing away with variable supply meant this knowledge was no longer required. Airborne radar was one step nearer a "black box". Secondly, the design finalised by Bowen and Metro-Vickers was adopted as a universal standard airborne radar power-supply by all the allied forces. This meant that there was a much greater degree of interchangeability so that sets could be upgraded or replaced, as all subsequent equipment was designed to run off this power source.

During the remainder of the year the group made further tests and improvements to the running of the transmitter and the receiver. At this stage the radar set still only gave range information, on the standard A-Scope display (using a time-base trace that gave the range as a spike on a line, read off on a scale, see figure 2.2).

The team were still deciding on how to accomplish attaining the required information, and then how to display it. The type of display used in ASV had already finalised, but as "chasing another plane in the dark is very different to spotting a slow moving ship", so

careful consideration had to be given about how to arrange the AI display.⁹⁴ The CRT screens used in radar displays had controls to operate tuning, brightness and focus. In the chain stations it required the operator to make further manipulations of controls to give bearing and elevation; range being read off the scale on the screen. For an operator in an aircraft, they decided that if possible bearing and range information would need to be displayed directly in an easily readable fashion to the operator. As Bowen wrote:

[T]here was no prior experience to indicate just what an operator could and could not do in the back of an uncomfortable and extremely noisy aircraft.⁹⁵

They eventually settled on the idea of using four receiving antennae; two mounted vertically and two horizontally. By separating the aerials in this way, the relative strength of the signal received in each aerial would indicate whether the target aircraft was above or below, to the left or to the right of the aircraft, by comparing this signal strength from the left-right and up-down pairs. This arrangement gave an easy method of display using two screens (see over for illustration).

They settled on this arrangement after rejecting two others that were too complex. The overlapping beam system employed in this method was already used in the Lorenz Landing System, something that would be familiar to the group members and hence would probably mean that it would be considered.⁹⁶ The methods that were rejected were the utilisation of the B-Scope (Range/Azimuth) display used in ASV instead of the twin screen method, and a method of phase comparison. The former was fine for ASV where height was not a consideration; the operators were searching for something on the 2-dimensional surface of the sea and a two dimensional display indicating how far ahead, and whether the target was

⁹⁴ Hanbury-Brown (1991), p26.

⁹⁵ Bowen (1987), p65.

⁹⁶ The Lorenz Landing System was a method of lining up aircraft to an airfield runway. Overlapping beams were transmitted either side of a runway, such that "dots" were heard on one side and "dashes" on the other. These dots and dashes were synchronised, so that when the pilot was in line with the runway he heard a continuous tone. This system became widely used around the world in the 1930's. A blind-bombing system based on this method was also used by the Germans in 1940. It was known as X-Gerät, and was successfully jammed by "bending the beams", sending the aircraft to bomb empty fields. One notable failure to achieve this was on the night of November 14th/15th 1940, when Coventry was bombed. See Jones (1978).

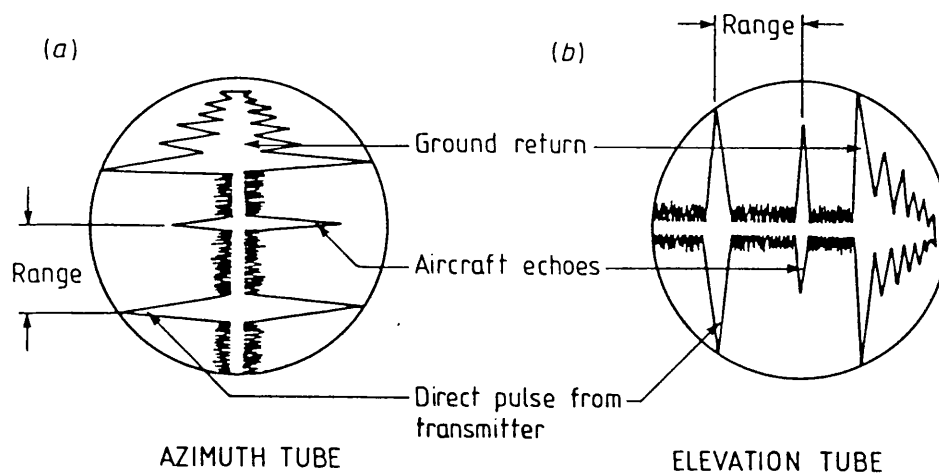


Figure 2.3 AI Marks I-IV display. The left hand screen shows that the target is to the right, and the right hand screen that it is above the aircraft. In each case the distance of the trace from the origin gives the range. There would be a range scale marked on the screen. Note the "ground return", in the form of a large spike. Anything in the area between the origin and the ground return is visible; if it is further away than the height of the aircraft above ground it is not visible. From Bowen (1987), p66.

to the left or right, was sufficient. The latter method involved finding direction by comparing the phase of the received signals; phase changing upon reflection from the object. However the method required sophisticated electronics, and Bowen didn't have the staff to spare on developing them.

— The display devised was as simple as could be made from existing knowledge. It gave the operator all the information he needed quickly and simply. They originally intended to use just a screen and display the information in the form of a cross. However this idea was abandoned as they considered that the large ground-return signal would create a messy area where the two traces overlapped, and make the display confusing.

Once they had decided on the display, their major task was to perform experiments to improve their apparatus. The next major hurdle for them was that of refining the transmitter to address the problem of the minimum range of the equipment. Bowen described what was they had to deal with:

It had always been recognised that the minimum range performance of an air-to-air radar would be crucial. At the time, the thought of firing blind at a target aircraft at night was simply out of the question. The Air Force made it an absolute requirement for a pilot to identify his target before opening fire. There would be just as many friendly aircraft in the air as there would be enemy aircraft and a high probability that a nightfighter would intercept a friendly aircraft... It was essential, therefore, that the minimum range should be small enough for the pilot to see and identify his target by eye before opening fire. In our discussions with the Air Ministry it was generally agreed that a minimum range of 1000 feet was the figure to aim for. This figure was based on a series of night tests that had been made at Farnborough several years before; they found that on moonlit nights a target aircraft could sometimes be seen up to 2000 feet away, but on really dark nights or on nights when the moon was obscured by cloud, a range of 1000 feet was required and that was the figure we aimed for.⁹⁷

Between 1938 and 1939 work progressed on this problem, albeit often slowly. The technical aspect of making a short enough pulse was relatively easy to solve, but protecting

⁹⁷ Bowen (1987), pp67-8. Night-fighting had long been an element of RAF thinking, even before the invention of AI radar, hence the trials to determine visual ranges at night.

the receiver from the strong transmitted pulse was not. However, Touch eventually did solve this, and they conducted tests to see how short the minimum range of the receiver was by using the height of the aircraft above the sea. They would compare the indicated ground return to the altimeter height, and by this method managed to successfully operate the set at minimum ranges of between 100 and 500 feet, which was perfectly adequate according to the Air Ministry 1000 feet criterion.⁹⁸

Unfortunately for Bowen, the whole question of minimum range became open during 1940 when production AI was being installed. Bowen believed that there was no problem with the work his team undertook:

[T]he minimum range question... was the cause of a misunderstanding later, when the airborne group was accused of having paid insufficient attention to this problem. Some of the production sets did not always meet the 1000 foot requirement, but this was a matter of adjustment, not of basic design.⁹⁹

I shall relate these difficulties in more detail in section 2.4.4.

During 1939 and 1940 the research set in the Anson was refined into firstly the Mark I apparatus installed over the Summer and Autumn of 1939 into Blenheims (aircraft that were in reality light bombers and totally unsuited to the task of night-fighting), and in October 1940 the Mark IV AI which went into the much more suitable Beaufighter. The Mark I was extremely troublesome both in operation and serviceability, and also because it took the researchers away from research into the task of installation, when, as war approached, the RAF insisted that the new device needed to be rushed into service (without proper trials or finalised design). As Hanbury-Brown recalled:

These demonstrations of AI [to VIPs such as Dowding and Churchill] were successful, in fact far too successful for its own good because they resulted in a complicated piece of equipment being introduced to the RAF in a hurry without provision being made for its maintenance and training. Before we knew what was happening we were committed to fitting 30 Blenheims with AI in one month for service trials by a nightfighter squadron (25

⁹⁸ Bowen (1987), p69.

⁹⁹ Bowen (1987), p69.

Squadron) at Northolt... In due course they were fitted but no further research on AI or ASV was possible for quite a long time.¹⁰⁰

A further interesting use of AI, tried out in February 1939, was for navigation. I mentioned that calibration was made by flying over the sea and relating the height given to that shown by the altimeter. Altimeters were normally set to show sea-level whilst flying, and were sometimes readjusted to show the height of an airfield above sea-level as zero, when the aircraft was on landing approach. Over high ground Bowen realised that it was possible to use the AI to read the height of the aircraft above ground using the ground-return, and compare it to the height above sea-level from the altimeter. Subtracting one from the other would give the height of the ground, and this could be compared to the contour heights on a map. Bowen made a flight navigating by comparing heights read off the AI to the contours on a map. This realisation that radar and map-making could be so joined was not utilised until much later, when it became the basis of H₂S (see chapter 5).

In addition to the technical deficiencies of the set, there were other problems to be faced before it could be used in action. It must be remembered that at this time many fewer people were familiar with items of technology at all, and that even the trained RAF fitters would not be used to the brand-new technology of the AI. Although operation was, after many flying hours, a simple procedure to skilled men like Hanbury-Brown, his expertise (both in operation and maintenance) had to be passed on to others:

Having got the AI sets into working order the first thing which I had to do was to teach the radio mechanics of 25 Squadron how to maintain and test them (not an easy thing to do without any instruction manuals or test gear). The next thing was to work out with the Squadron how best to use an AI set in the air, which we did by making innumerable mock interceptions with one or two of the pilots and ourselves as AI operators.

Having established a reasonable procedure, we then had to train all the pilots and radar operators to carry it out. We started on the ground by showing the men who had been chosen by the Squadron to be trained as radar operators how to work the set and how to tell the pilot the distance and direction of the target using the intercom.¹⁰¹

¹⁰⁰ Hanbury-Brown (1991), p28.

¹⁰¹ Hanbury-Brown (1991), p36.

It was only when these preliminary stages were carried out, and finally the techniques for operation were decided upon and being trained for, that the pilots, operators and the researchers involved with them were able to see how well their system worked. During the initial training period, interceptions were made in daylight and the pilot picked up his target visually, saying nothing whilst the operator used the AI apparatus to direct him onto the target. This was difficult enough in itself, and involved careful judgement of the closing speeds of the two aircraft and the ability to communicate directions clearly to the pilot. However, when air interception was tried out at night, it was discovered that it was practically impossible for ground operators to place the nightfighter behind the target and within range of the AI, as CH and CHL were not up to the task:

What everyone, including Fighter Command, had failed to foresee - until it was almost too late - was that AI by itself, with its limited range, was only half the solution of the problem of guiding the fighter onto the bomber in the dark; the other half of the problem was to build a new type of radar for ground control.¹⁰²

Hanbury-Brown wrote up the results of his investigations, conducted at Northolt airfield during October and November 1939, and passed them onto Rowe at Dundee. He recommended a form of Ground Control using a sweeping narrow beam, with the display similarly using a rotating line to show indicating blips of target aircraft in relation to the centre, which was the location of the radar. This form of display, known as a Plan Position Indicator (PPI) is widely used today, but in 1939 it was a revolutionary idea.¹⁰³ Rowe set wheels in motion and in October 1940 the GCI (Ground Control Interception) Station was introduced. This incorporated the rotating PPI, which facilitated the passing of accurate positional information between the ground controller and the night-fighter crew. The PPI was also later used in H₂S, as a rotating map display (see chapter 5). The techniques worked out using AI Mark I, and its replacement Mark IV, were also used with the centimetric AI, Mark VIII, introduced in 1942 (see chapter 4).

¹⁰² Hanbury-Brown (1991), p41.

¹⁰³ Unfortunately Hanbury-Brown did not state how he came upon this idea.

What is important to draw out of this is that new technology will have unforeseen effects, both counter-productive and beneficial. Despite having embedded their skill into the prototype AI, it still took someone skilled and familiar with its operation to teach others, *by personal contact*, how to maintain and operate it. It also shows that any new technology will fit into the cultural framework¹⁰⁴, but at the same time alter the “rules of working”. These alterations in established procedures can also highlight areas that require further change for the new technology to be effective, in much the same way that, for example, the introduction of steam-engine technology altered approaches to engineering embankments, cuttings, viaducts and so forth. When the first steam locomotives were introduced they were unable to cope with the steep inclines that the colliery railways used. The “playing field” had to be levelled for them, by putting railway-lines on gentle slopes. This necessitated the building of spectacular civil-engineering projects, such as bridges, viaducts embankments and cuttings. This process was at work in the field of radar in 1939/40. The introduction of new equipment and practices necessitated a change in some of the old equipment and practices in order for the full potential of the new to be realised. It was only by actually using the equipment that the need for these changes became evident; they were not foreseen, and one could argue that they were not foreseeable.

2.4.3 Anti-Surface Vessel (ASV)

As I indicated in the previous section, one of the first things noticed by Bowen and team when they first took to the air in 1937 with their airborne apparatus, was that ships stood out clearly from the surface of the sea. During exercises in September of that year, Bowen was able to locate the North Sea Fleet in dense fog with the radar-equipped Anson when the other Coastal Command aircraft were grounded. He also used the equipment as an altimeter and coast-detector; the coast, too, standing out on their apparatus. Due to these successes emphasis was shifted from AI to ASV for most of 1938. During this period a lot was

¹⁰⁴ The idea of technology being shaped by, and at the same time shaping, strategic goals in wartime is discussed in Beyerchen (1994), and in chapter 6. By cultural framework, I mean the general setting of accepted practice and apparatus being used at a particular time and place.

learned about how to build and operate airborne equipment, as the targets used for the calibration were slower and larger objects than aircraft. After priority was switched back to AI, development of ASV continued in parallel to AI, using the same 1.5m equipment, and ASV Mark I was introduced into service at about the same time as AI Mark I.

Britain, being an island, depended heavily on shipping for supplies. When the war began in 1939, Bowen was called on to see whether the ASV Mark I would be able to detect submarines. The Submarines of the period needed to surface regularly both to recharge their batteries, and to enable speedier travelling to and from bases to hunting-grounds (submerged speed was only 5 knots, using electric battery-powered engines; surface speed was 15 knots when diesel engines were used). In each case when surfaced, the ships were visible during daylight hours and therefore vulnerable to attack, which meant that both these operations were normally conducted at night. Hence if ASV could be employed, then submarines could be attacked at any time. The major question was whether a submarine conning tower would offer a large enough target for ASV. An exercise with an RN submarine was conducted during November 1939, and Bowen successfully located it when only the conning tower was above water.

ASV was used throughout the war, and a centimetre version based on H₂S was also introduced in 1943. It plays a relatively minor part in my account because the innovations that I am interested in were mainly related to developments in AI radar. However, the debate about allocation of resources into AI or ASV, and later H₂S or ASV, depended upon the state of the Battle of the Atlantic (or the U-Boat war) at that time. The success in aircraft locating and destroying fluctuated during the war, and was dependent on more than just ASV, but figure 2.4 shows the impact that it and other devices had (see over). In general, rises in sinkings caused panic and prompted a “Something must be done” attitude from senior government members. As can be seen from the graph, ASV Mark I was particularly unsuccessful for reasons that were very similar to the failure of AI Mark I, as I described in the previous section. Bowen related that:

The failure of attacks during 1940 can be put down to many factors, but in simple terms it was due to the need for a very protracted training period, during which the crews

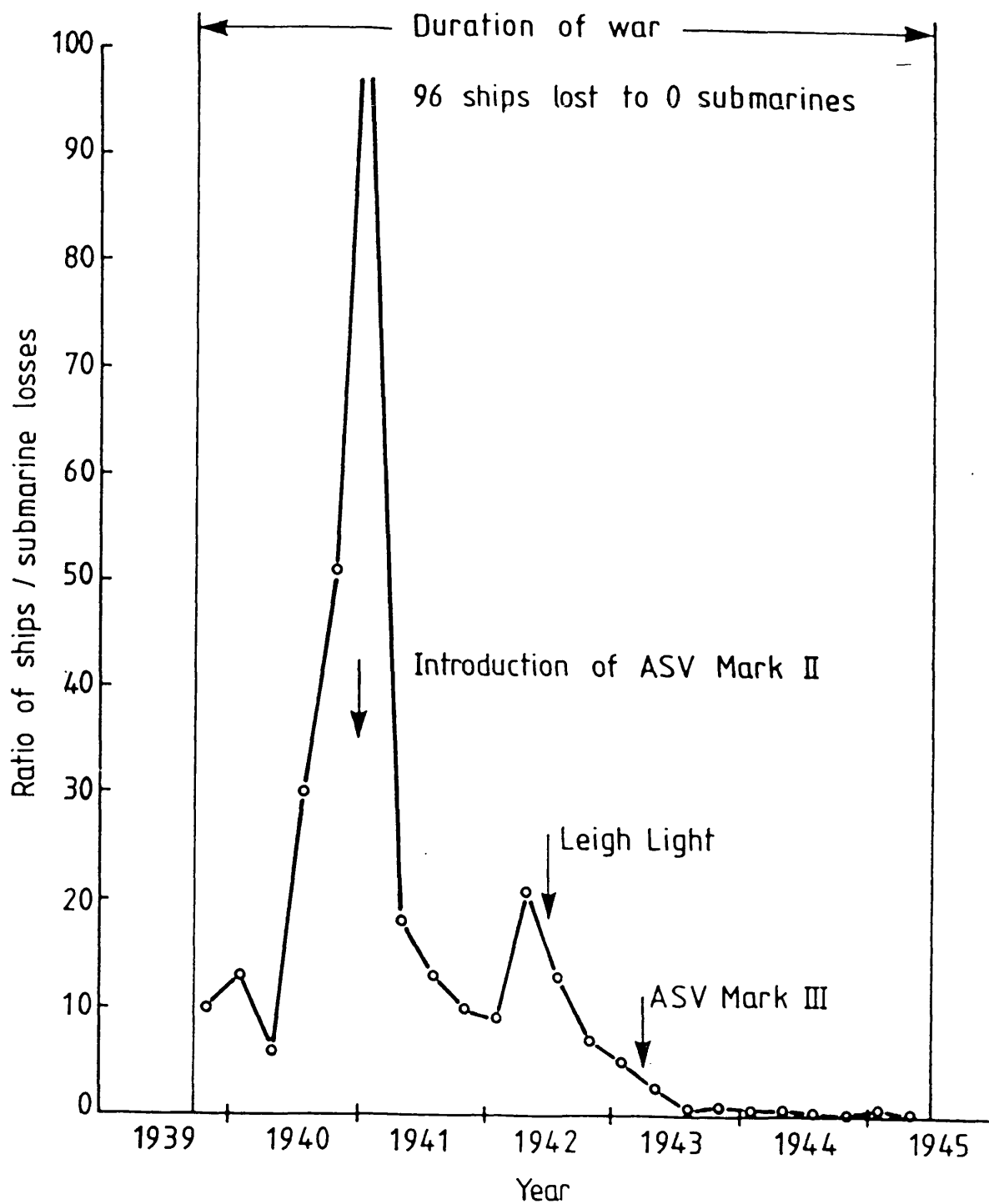


Figure 2.4: The effects of ASV. The ratio of Allied ships sunk to the number of German submarines lost, plotted at three monthly intervals for the duration of the war. From Bowen (1987), p109.

of Coastal Command and the Fleet Air Arm learned how best to use their new equipment. Their problems were compounded by the poor serviceability of ASV Mark I, the lack of test equipment and the total absence of training facilities. It remains true to this day that any military service, when faced with the introduction of a new and exotic equipment, requires meticulous training and a long and protracted practice period before they can make the best use of it.¹⁰⁵

An improved ASV, Mark II, was introduced in Autumn 1940. The primary effect of having a more serviceable machine was that crews operated longer hours and made more attacks on submarines, along with more sets being in service, and this forced submarines to be more wary, hampering the U-boats' ship-hunting activities. Shipping losses fell initially, but when U-Boat commanders got more used to the tactics, they rose again.

The introduction of the "Leigh Light", an aircraft-mounted searchlight, tipped the balance in favour of the allies again to the point where the U-boats were forced to change tactics, preferring to surface during daylight when aircraft could at least be spotted. However, in late 1942 the Germans finally introduced a warning receiver on 1.5m, a move that came about after Rommel's forces captured¹⁰⁶ an intact ASV-equipped Wellington in North Africa. Unfortunately for the Germans, the warning receiver came into service only shortly before the introduction of 10cm Mark III ASV. They had developed a warning receiver for 1.5m ASV Mark II, which was just being replaced by the new 10cm ASV. Naturally, they were unable to detect the 10cm ASV-equipped aircraft until they discovered the usage of it in February 1943, and developed a suitable warning receiver. This did not occur until the Battle of the Atlantic was effectively won.¹⁰⁷ This final variant was adapted from H₂S, and was the product of a further political wrangle between Tizard, who favoured emphasis on the U-Boat problem, and Lindemann (by now Lord Cherwell) who leaned towards night-bombing aids. The details of this are in Chapter 5.

¹⁰⁵ Bowen (1987), p109.

¹⁰⁶ This was another example of equipment capture by the Germans. It came only a couple of months before the capture of centimetre H₂S, so had little time to make an impact (see chapter 6 for more discussion of German pre-centimetre airborne radar).

¹⁰⁷ Bowen (1987) makes mention of the Germans being fooled by a captured operator into thinking infra-red emissions were the reason for their 1943 U-Boat losses, rather than 10cm ASV. By the time they realised this was the case (mid/late 1944), the war was effectively over.

Between mid 1943 and the end of the war, the Germans never regained the initiative in the Atlantic, despite eventually developing a 10cm warning receiver after the discovery of this development (described in Chapter 7). I make mention of the ASV developments as background information; the events, whilst important to the course of the war, are not a major part of my thesis.

2.4.4 The Problem of Minimum Range

When war broke out the whole Bawdsey establishment was evacuated from their Suffolk manor house up to Dundee University. The reason for this was the proximity of Bawdsey to Germany; it was feared that the Establishment would come under attack immediately. This move caused chaos on two fronts: firstly due to the physical problems of moving a whole research establishment lock stock and barrel, and secondly because the arrangements for the move were not properly made, so Dundee University were unprepared for receiving the radar men.

On arrival at Dundee in early September 1939, the airborne group were further isolated by being sent to Scone airport some twenty miles distant. Scone was a small grass field, with no facilities apart from a small hut, and was clearly totally unsuitable for building and testing airborne radar equipment. Unfortunately the remedy, when it came in November 1939, was little better. The group were dispatched to St Athan, near Barry in South Wales. They had more space, but in unheated hangars during the middle of the worst winter for fifty years, and were also on an operational airfield which was bombed at one point. As Lovell pointed out:

The fallacy of our emergency move from Bawdsey and the aerodrome at Martlesham Heath was soon underlined a few weeks after our arrival at St Athan. A JU 88 [German bomber] appeared out of the murk and dropped a 1000 bomb on the main runway. The story of AI radar and Bowen's group might well have been different had this bomb

exploded, but it did not do so and proceeded to bounce along the runway like a tennis ball - the detonator was faulty.¹⁰⁸

Now the airborne group were some 300 miles distant from the parent establishment, and were also reduced to the role of mechanics installing AI into 25 squadron Blenheims. During this period very little research work was actually done, and the airborne group also became personally isolated from the hub, especially Bowen. Lovell remarks that “very few of us who arrived at St Athan in November 1939 ever did return to research at the parent AMRE establishment”, and it is not hard to see why (I cover Lovell’s personal account of conditions in chapter 4, along with more detail about why the move took place and who authorised it). The move was a disaster for AI development, and the main reason for this occurring was the so called “problem with minimum range” of AI.

Early in 1940 great dissatisfaction arose within the RAF with the then current AI, Mark III. This was because AI-equipped Blenheims were failing to shoot down anything at all. There were several reasons why this was happening, and it took a thorough investigation to work out why this was so. This was not before it had led to the alienation of Bowen from the rest of the establishment. It also illustrates how scientific results that are thought resolved (such as that AI can “see” other aircraft), can become contentious again. The argument made against AI by the RAF was that it didn’t work because the minimum range of the system (1000 feet) was too low. They insisted that only a change to the equipment’s design would help operators to “see” and shoot down enemy aircraft at night.

During air testing, the RAF had not found the new Mark III set satisfactory, and their complaints were summarised by Hodgkin as follows:

1) Massive ground echoes made it impossible to detect aeroplanes at a range greater than the height above ground. This had come up in an acute form because it had proved impossible to intercept the low-flying mine-laying aircraft which had been used extensively during the winter nights of 1939-40.

¹⁰⁸ Lovell (1991), p21.

In some ways it is strange that the RAF made mention of this, for they had long known that maximum range was a problem with 1.5m AI. This is why Bowen was so keen to go for 10cm in the first place.

- 2) The direction-finding arrangement was somewhat unreliable and could not easily be tested on the ground. (The reliability of Mark IV AI was much better than that of Mark III.)

The main problem here was the arrangement of the aerials on the aircraft and the polarisation of the system, problems addressed and solved by Hanbury-Brown as I will describe shortly.

- 3) The display was difficult for a skilled observer and impossible for an unskilled one.

This problem was one that would not only bedevil AI, but was also a problem for other types of airborne radar like ASV and H₂S (see chapter 5). Interpreting what was on the screen of the display and relating it to an object “out there” was something that became, after practice, unproblematic for the scientists who devised the radar sets. However, their interpretative skills had to be taught to the RAF operators, many of whom were not familiar with electronic apparatus. Some learned these skills easily, some with difficulty, and some not at all.

- 4) The minimum range was too long and the maximum range was too short. The RAF wanted something less than 500 feet for minimum range and greater than five miles for maximum range...

- 5) The RAF wanted an equipment which could be installed in a Spitfire¹⁰⁹ and would be used by the pilot rather than by an observer. They favoured a ‘spot-indicator’ type of display on a single cathode ray tube.

- 6) Aircraft manufacturers did not like cluttering up the wings of high-performance aeroplanes with aerials of any kind.

¹⁰⁹ This was a single-seat, single-engined fighter. In fact AI for single-seat aircraft was something never used during the war, although the technology to make such a device existed. The original limitations of the 1.5m AI forced the adoption of the two-seat principle (pilot and radar-operator). Once this system was found to be successful, it became the operational norm.

7) It would be easy to jam a wavelength of 1.5 metres.¹¹⁰

Most of the problems that Hodgkin gives were current in the minds of the AI team at the time. Bowen believed that some of them were just inherent problems of using 1.5m, and that they could all be solved by using 10cm (see chapter 4).

In early 1940 Hanbury-Brown was posted to the newly formed Fighter Interception Unit, that was formed to help solve the problem of why AI was proving so unpromising in service. He summed up the problems with AI neatly:

If our fighters were really going to shoot down enemy aircraft at night a great deal more experimental work would have to be done and new equipment, such as GCI radar, would have to be developed. Of course many things were needed besides GCI, such as faster aircraft with better armament, better radio communications, a system of identifying friendly aircraft to the GCI, a more reliable model of AI and better test gear. As far as organisation was concerned there was an obvious need for better training of radar operators and mechanics.¹¹¹

One of the first problems that Hanbury-Brown dealt with was that mentioned by Hodgkin in point two - that of unreliable direction-finding. The original AI Mark I was fitted to Blenheim aircraft with a long nose. The equipment was thoroughly tested using this aircraft. By the time Mark III was tested, the design of the Blenheim had been changed to one that had a much shorter nose. Pilots using the new set up (the short-nosed Blenheim *and* the new AI) complained about its performance. However, Hanbury-Brown identified that the main problem was one of serviceability. Quite simply, the sets kept going wrong because they were hurriedly made. There was also another problem that was not attributable to operators complaining about the performance:

We spent hours and hours at 20 000 feet measuring everything that could be measured and much to my surprise, we did find a really serious fault. We discovered that when the angle of elevation, or depression, of the target relative to the Blenheim was less than about

¹¹⁰ Hodgkin (1992), pp145-6.

¹¹¹ Hanbury-Brown (1991), p55.

45 degrees its azimuth was shown correctly, but at angles greater than 45 degrees the display of the azimuth was not only wrong in magnitude but was sometimes wrong in *sense*. Above an elevation of about 60 degrees a target on the *left* of the fighter was actually shown as being on the *right* and vice versa!¹¹²

The RAF operators, being relatively unskilled in AI operation compared to Hanbury-Brown, knew that what they were getting was wrong, but they were unable to tell why. Hanbury-Brown, with his years of skill in operating AI radar, knew that he was able to “repeat the experiment” to get the “correct result”, so he also knew that in this case there must be something wrong with the apparatus and not with the operator (himself). He was aware that a lot of work had gone into *improving* the original AI Mark I to the stage of Mark III. He believed that the Mark III electronics should be more reliable than their Mark I predecessor, which meant that there were only a few possibilities left as to where the problem lay. He set about testing the antenna set-up:

It didn't take me long to find out what was the matter. The original installations of AI had been made in long-nose Blenheims and the performance of their azimuth antennas had been thoroughly tested at Martlesham and Northolt. But the later installations had been made in short-nose Blenheims and we had used the same antenna system. Although in routine tests at St Athan it had given satisfactory patterns in the horizontal plane, we had failed to notice that at high angles of elevation, or depression, there was a strong signal reflected from the cowling of the engine nearest to the antenna. On a short-nosed Blenheim this unwanted reflection seriously modified the directional patterns of the antennas and at very high elevations actually reversed the apparent azimuth of the target.¹¹³

After investigating the problem, Hanbury-Brown knew that it was the new arrangement of antennas that was unsatisfactory. They needed to be changed in order to get the Mark III AI working satisfactorily. He was able to discover this due to his possession of two types of skill. Firstly, he possessed interpretative skill from flying with the AI apparatus. He knew

¹¹² Hanbury-Brown (1991), p58.

¹¹³ Hanbury-Brown (1991), p58.

how to associate what he saw on the display with an aircraft being pursued. He could, therefore, identify when this relationship was not working properly (giving the correct result). Secondly, because he had a large degree of embodied skill that he had built up by actually designing, building and modifying the equipment through years of interaction with it and what it produced, he was able to identify the source of the trouble much quicker than anyone else. In this second case he possessed tacit knowledge (in the form of embodied skill) about what could be the source of the problem. As he was one of the men who had embedded his radar-building skill into the AI “black box”, he was also able to unpack this black box quicker than anyone else because of his tacit knowledge. Importantly, he was also able to make a step that solved the problem, as he describes:

After a few hasty and unsuccessful attempts to reposition the antennas, I took the bull by the horns and changed the polarisation from horizontal to vertical. This completely cured the trouble; we were now able to design remarkably neat antennas which could be mounted far away from the engines where the troublesome reflections from the engine cowlings would not reach them.¹¹⁴

The key phrase to note in this paragraph is “I took the bull by the horns”. This to me suggests that it was a possibility that Hanbury-Brown (or one of the other members of the airborne group) had already thought of as a possible next-step towards improving the apparatus. Again this is, I believe, an indicator that Hanbury-Brown possessed embodied knowledge about AI radar. The way he words it, it appears that changing the aerial polarisation was an *obvious next step*. However, such a step would probably be far from obvious to an electronics engineer unfamiliar with the behaviour of radio waves transmission and reception in airborne radars.

Unfortunately, at the same time as Hanbury-Brown was dealing with the directional problems, Bowen came under attack for what was suddenly perceived by the RAF to be a further problem with AI: the minimum range of the equipment. As I have quoted earlier, the RAF had many misgivings about using AI at all, due to the extremely poor results of the first

¹¹⁴ Hanbury-Brown (1991), p58.

few months in service of the equipment, when very few enemy aircraft were located and none were actually shot down.

At the time that the argument arose, the main establishment was based in Dundee, but the airborne group were at St Athan in South Wales (as described in chapter 4). The physical distance alone was enough to open up a chasm between the airborne group and the main site, but the nature of their work (fitting AI, not doing research) exacerbated the problem. Not only was there the physical difficulty of communication between two distant points, but there was also a problem over personalities. This arose because Rowe appointed W.B.Lewis, a Cavendish physicist, to be his deputy ahead of many of the Bawdsey radar pioneers, one of whom was Bowen. As Lovell recalled:

[I]t is hard to understand why Rowe allowed one of his key groups to be destroyed in this way. An important factor was no doubt the serious acrimony that had developed openly between the Rowe - Lewis grouping and Bowen. The genesis of this was the arrival at Bawdsey of Lewis in the summer of 1939. At that stage Bowen and Wilkins [Watson-Watt's former deputy] were the senior and pioneer members of the research group - Bowen on airborne radar and Wilkins on the development of the main ground installations. However, Lewis impressed Rowe to such an extent that on the outbreak of war and the move to Dundee, Rowe 'had risked offending my old colleagues by appointing Lewis as my deputy although he had been with us but a few months'. In fact, Rowe did not avoid this risk. Wilkins soon moved to Watson-Watt's London Office and Bowen was forced into increasing isolation, first at Scone [Airport, near Dundee] and then St Athan.¹¹⁵

Rowe, who took over as superintendent from Watson-Watt in 1938, was a civil servant and a former bomb-ballistics researcher. He was not familiar with electronics, but Lewis was. This is presumably why Rowe recruited and promoted him: to act as an "expert" to back him up. However this action certainly infuriated Bowen, who did not think much of Lewis' abilities with electronics and his (or Rowe's) understanding of airborne radar (see

¹¹⁵ Lovell (1991), p21. See also in chapter 4, where I describe the relationship between Lewis, Rowe and Phillip Dee who took charge of the centimetre research group in May 1940.

Chapter 4)¹¹⁶. Bowen was also irritated by Rowe's lack of flexibility as a manager, and he was not alone in this respect.¹¹⁷

As I mentioned earlier, the criterion that Bowen, Hanbury-Brown and the rest of the group took as their benchmark for the minimum range performance of their AI was 1000 feet. This target was met, as far as they were concerned, when they designed their system, as Bowen recalled:

[T]he minimum range question... was the cause of a misunderstanding..., when the airborne group was accused of paying insufficient attention to this problem. Some of the production sets did not always meet the 1000 foot minimum range requirement, but this was a matter of adjustment, not of basic design.¹¹⁸

So Bowen was stating that any flaws in the performance of the operational sets were caused by poor manufacture, not poor design. Hanbury-Brown noted that the production sets didn't always meet the requirement either:

Our new version of AI (AI Mark III), like its predecessors, did not always meet this requirement; its minimum range might be anything between 800 and 1500 feet depending on how the receiver had been adjusted.¹¹⁹

Like Bowen, he also asserts that "in the airborne group we knew that this figure could be reduced to about 800 feet by a simple modification of the receiver... My own view... was that a minimum range of 1000 feet was adequate and I doubted whether night-fighter crews could make use of anything else."¹²⁰ Another point to note is that Hanbury-Brown was again alluding to the competence of him and the other members of his group in their ability

¹¹⁶ Lewis provoked varied reactions from the people who knew and worked with him, as the letters written to Lovell when he was writing Lewis' obituary show (see W.B. Lewis papers). On the positive side, Lovell believes that it was Lewis' intervention that saved 10cm AI during the summer of 1940 when there was considerable pressure to divert all resources into meeting the immediate threat of German invasion.

¹¹⁷ See Lovell (1991), Batt (1991), Atkinson (1990), Hanbury-Brown (1991) and Hodgkin (1992) for examples of Rowe's particular eccentricities regarding the behaviour of his research staff.

¹¹⁸ Bowen (1987), p69.

¹¹⁹ Hanbury-Brown (1991), pp59-60.

¹²⁰ Hanbury-Brown (1991), p60.

to build working AI radars. This is further evidence that they were aware of their own embodied skills in this respect.

The RAF's general dissatisfaction with the performance of AI was relayed to the main establishment at Dundee. Rowe consulted with the RAF's Operational Research Section. Its head, Harold Larnder, told them that the main difficulty with AI was that the minimum range of the set was unsatisfactory, and that it should be more like 600 feet than 1000. When Bowen was informed he was considerably irked, as according to him Larnder "knew nothing about the subject, had never been connected with airborne radar at Bawdsey and... never [took] a flight to see for himself."¹²¹ He knew that Larnder did not possess the same amount of embodied knowledge that he had gained through four years' work with AI radar, and believed Larnder was in no position to make these sort of decisions.

Lewis, Rowe's deputy "decided to work on it for himself"¹²² without apparently consulting any of the airborne group. He had an idea for changing the pulse-shape to make it sharper and shorter, reasoning that this would then reduce the minimum range of the set. He gave the job to E.H. (Ted) Cooke-Yarborough, a "very competent electronic engineer"¹²³, and also to engineers at EMI. However (according to Bowen) EMI were "a very expert group, but were as inexperienced in airborne matters as [Lewis] was."¹²⁴ Hanbury-Brown's experience with the engine-cowling problem and aerial polarisation lends weight to this view.

Both Bowen and Hanbury-Brown believed that the problem of minimum range was one that was created by Lewis and Larnder, something that they as competent airborne radar researchers of several years' standing had already assessed and passed over as in hand. As far as they were concerned, any problem of minimum range either didn't exist, or was due to errors in manufacture, or to the set-up of the whole night-fighting organisation. Both men, as skilled engineers, had arguments to back up their position. Bowen believed that Lewis and Larnder didn't understand how the receiver worked; they didn't have the skill that he had through having built it:

¹²¹ Bowen, letter to Lovell 2/5/87. In W.B.Lewis papers.

¹²² Hanbury-Brown (1991), p60.

¹²³ Hanbury-Brown (1991), p60.

¹²⁴ Bowen, letter to Lovell 2/5/87. In W.B.Lewis papers.

What both Larnder and Lewis failed to realise was the simple fact that in considering the minimum range performance of a radar set, there had never been a problem in getting the pulse width down. The whole problem was due to ringing in the receiver which in our case was within a few feet of the transmitter and was subjected to a tremendous kick when the transmitter fired. This was well known and well understood from the early thirties, when Wilkins and myself were familiar with the ionospheric work going on at Slough and King's College. We carried this knowledge to Orfordness in 1935 and successfully introduced some of the precautions needed into the first air warning set. Neither Larnder nor Lewis were involved in this work and as I have said, they got the problem entirely wrong; if there was a problem it was due to ringing in the receiver and not due to the transmitter.¹²⁵

In this passage Bowen also mentions the transportability of his embodied knowledge. He and Wilkins learned about ringing in the receiver at the Slough research station and were able to transport their embodied knowledge with them as they moved. It is possible that this knowledge could have been made explicit if required in the same way as the TEA-Laser building knowledge was made explicit in Collins' study.¹²⁶ But if Bowen and Wilkins were not around, they would have been unable to give this embodied information to their colleagues.

Bowen's embodied skills in building airborne radar were no use to him in this debate, as he was well removed from the centre of what was happening by being in South Wales. The status of Larnder and Lewis, who were senior in the civil service hierarchy to Bowen and Hanbury-Brown enabled them to question the solutions to airborne radar problems made by Bowen. However, according to Bowen they didn't have the necessary competences to ask the right questions about the lack of performance. Hanbury-Brown said the same things of Cooke-Yarborough:

¹²⁵ Bowen, letter to Lovell 2/5/87. In W.B.Lewis papers.

¹²⁶ See Collins (1985), ch3.

[Lewis and Cooke-Yarborough] made the first test flight from Leuchars and found the results were inconclusive partly because of the difficulty of independently measuring the range at which the echo signal disappeared.

I found this mildly amusing as I had already discovered that it is no use trying to estimate the distance from one aircraft to another when you are in the air, it has to be measured. In the many mock interceptions which I made when demonstrating AI, everyone without exception, including experienced pilots, underestimated the distance between aircraft by a factor of at least two. To most people an aircraft 500 feet away looks dangerously close! At Martlesham we solved this problem by flying low over the sea and using the aircraft's altimeter to calibrate the minimum range of the echo from the surface of the sea.¹²⁷

On moving to Swanage (see Chapter 4), the transmitter designed by EMI "electronics wizards" Alan Blumlein¹²⁸ and E.L.C. White was air-tested along side Cooke-Yarborough's own version of an improved transmitter. These tests showed that the EMI version gave a greater maximum range, both having a similar minimum range of around 500 feet. The EMI transmitter was incorporated into AI Mark IV along with Hanbury-Brown's vertically polarised aerial system. This system was eventually installed into the much faster Beaufighter twin-engined night-fighter in Autumn 1940. When combined with GCI (introduced in October 1940) and with several pilots and radar operators who had virtually taught themselves how to make interceptions, the new system made a much greater impact.

The "problem of minimum range" was made into an artefact of the apparatus by Lewis and Larnder. They were both unfamiliar with AI, who had never flown with it and who hadn't spent several years building it and experiencing it in operation. Bowen and Hanbury-Brown, both far more skilled than the other two were convinced that any failure of their apparatus in operation was due to factors other than their design. The upshot of this dispute was a fine version of AI, mark IV, but the downside was that the rift between Rowe and Bowen became irrevocably large. As Hanbury-Brown says: "In effect Taffy [Bowen], who

¹²⁷ Hanbury-Brown (1991), pp60-1.

¹²⁸ Blumlein was part of EMI's television team, and recognised as the finest electronics engineer of his generation. He later worked on H₂S, and was tragically killed in June 1942 when the test aircraft he was flying in crashed.

had been one of [radar's] brightest stars was lost to AMRE"¹²⁹. He became sidelined at Swanage and all his skill and experience was lost to the members of the centimetre group. This occurred when Tizard asked him join him on his trip to America in September 1940. Bowen's responsibility was explaining the prototype cavity magnetron (a type of 10cm transmitter valve, see chapter 3 and 4) to American scientists and engineers.

I think the general point to draw from this is that it is impossible to know what kind of impact Bowen's airborne radar skill would have made on future developments. Lovell speculated that Bowen may have been able to speed up development of H₂S in 1941/2 (see chapter 5). What one can say is that his skill appears to have been correct in defining the minimum range problem as an irrelevance to the problems of AI in general:

I once did an assessment of the minimum ranges actually recorded by Fighter Command pilots in combat reports in 1940 and 1941... in actual combat, the median minimum range to which enemy aircraft were tracked by AI and then seen by eye was between 1200 and 1500 feet... When there was a moon or the target had been damaged or had defective flame compressors, the range at which the target was sighted was sometimes over 2500 feet. There were few reports, if any, below 1000 feet. These figures are remarkably close to the RAE results of five or ten years earlier. So much for Larnder's claim about the defective minimum range of AI! His claim was doubly phoney. Not only was the original performance of AI well within the Fighter Command requirement, but as was demonstrated by the night battles, there was no need for anything else. The whole thing was a fabrication of Harold Larnder, and neither he nor W.B.Lewis knew the technicalities involved.¹³⁰

That Bowen was ignored and sidelined was regrettable, and may have had as I indicated an adverse effect on radar. The minimum range issue shows that a researcher's status within the hierarchy of an institution can confer the holder with the power to judge what is "correct" and what is not. This power can be wielded to overrule those who have built up a superior level of knowledge through their practical experience of apparatus. Lewis was deputy Superintendent of the radar establishment and Larnder was head of Operational

¹²⁹ Hanbury-Brown (1991), p61.

¹³⁰ Bowen, letter to Lovell 2/5/87. In W.B.Lewis papers.

Research at Fighter Command. This status meant that they were deemed better able to decide the nature of a problem concerning radar than the people who had the practical hands-on skill of making the device.

2.5 Conclusion

In this chapter I have done four things. Firstly, I have given a brief history of radar development up to the final variant of conventional technology AI. Secondly, I have given an account of some of the major steps taken whilst building radar in the ground and air forms in Britain just before the second world war. Thirdly, I have tried to unpack the contentious nature of radar research, to show how not everything was clear cut and how many people remained to be convinced that the developments that seemed obvious to the pioneers actually did what they claimed to. Lastly, I have brought out the vulnerability of the enterprise to personality conflicts and changing location.

There are several things to draw out of these points. The embodied skill of the airborne pioneers in building their radars was considerable, and this skill had to be propagated out into the wider world in several ways if airborne radar was to be a success. Their embodied skill had to be black-boxed, or embedded, into the production AI in order to make sure they performed as they should. This was the issue of actually building production equipment that performed as well as the laboratory sets. In the case of Marks I-III the pioneers acknowledge that production was not up to scratch, but that in the beginning there were:

[I]nadequate engineering standards in industry - the manufacturers were good people who improved fast, but when they started, they only knew the technology which went into domestic radio receivers.¹³¹

¹³¹ Bowen, letter to Lovell 2/5/87. In W.B.Lewis papers.

The second skill which, even when adequate equipment was developed in the form of AI Mark IV, was the ability to “see” anything using the equipment, and to use that ability to shoot down enemy aircraft, for that was the measure of success for AI. It was only by a great deal of trial and error that all the conditions for performing this successfully were worked out:

Most of the successful night-fighter pilots of 1940 and 1941 were entirely self-taught. This was not the fault of any one person, but was due to the fact that a piece of equipment was being introduced into service for the first time. By comparison, the introduction of centimetre wave equipment a few years later was a good deal easier because there were plenty of people in the RAF who knew the broad principles involved.¹³²

Another thing to note is that with AI we see the emergence of the type of team that was to dominate British radar research. This was a small team of people with good practical and experimental skill, who worked closely together and were able to develop a good working relationship. Rapid results brought continued success, and a belief in the project. They built up a large amount of embodied and embedded knowledge of how to build and operate airborne radars; what skills were needed to “see” objects using the screens in the aircraft, and how to fix things if nothing could be seen. Hodgkin described how useful one of these walking repositories of radar-building knowledge was to him:

I... learnt more about radar and electrical engineering from [Hanbury-Brown] than from anybody else.¹³³

Fortunately Bowen was around to point people in the right direction when most of the centimetre components and the early centimetre radars were being designed and built. He was lost to the centimetre team after that, in September 1940.

Finally, we have seen that this sort of team was sensitive to disruption in the form of personality clashes and also in geographical dispersal. There is a great difference between

¹³² Bowen, letter to Lovell 2/5/87. In W.B.Lewis papers.

¹³³ Hodgkin (1992), p142.

being committed to an idea about how to do something and disagreeing about details of how to do it, and disagreeing about the fundamentals of how to do it. In this case, and later on with the centimetre research, the successful teams (in terms of quick problem solving) had very strong common bonds and worked together in small spaces. This appears to have provided the optimum conditions for sharing embodied knowledge in the form of practical skills and also in terms of ideas about how to progress. These ideas were usually generated by having an intimate knowledge of the apparatus. The ability to consult quickly with colleagues who shared this knowledge, or possessed greater knowledge, facilitated quick progress. Geographical and personal isolation inhibited this speed. In the next two chapters I will describe firstly how similar small, highly skilled teams developed components for centimetre radar, and then how they combined these components into a 10 centimetre AI which got rid of the problem of *maximum* range of AI. Before that, I shall briefly consider how things could have gone.

A Counterfactual Intermission

By late 1940 the Mark IV AI finally bore fruit and was used very successfully for the next two years until the introduction of centimetre AI. Similarly, the 1.5m ASV also served its purpose very well. The other major uses of centimetre radar were in bombing, and these applications grew out of the apparatus being developed for centimetre AI. Centimetre radar was considered a vital research direction to take, and the occurrences of this story are what I examine in this chapter. Before I undertake that task, I wish to consider briefly what other directions the story of radar development could have taken.

The main reason that the development of centimetre radar was undertaken was because of the problem of the maximum range of the 1.5m AI. Bowen believed very strongly that 10cm radar was the only solution to this problem. However, in practice, 1.5m (Mark IV) AI was found to work perfectly adequately when used in conjunction with a proper fighter-control (GCI) and skilled pilots and radar operators. As events turned out, this information was only discovered well after the development of centimetre radar had commenced. However, if maximum effort had gone into AI during 1937/8 (rather than effort being diverted into ASV for a year), it is plausible that the problems of AI would have been solved a year earlier. This would have negated the pressure for centimetre AI, and quite probably stalled the research project.

Without a perceived need for AI the pressure for centimetre research would have removed. This is infact what happened to some extent in Germany. Centimetre research involved exploring a whole new field of physics. As I discuss in chapters 6 and 7, German metric radars performed the tasks envisaged for them very well. It was only when the Germans had a pressing need for improving the performance of their radar that they undertook centimetre research. however, they had also by this stage captured British centimetre radar, and felt obliged to catch up in this area.

It is interesting to speculate what would have happened had centimetre radar not been developed. Probably the most significant change would have been in terms of the Battle of the Atlantic, where people such as Rowe have argued that 10cm ASV made a crucial difference during early 1943. During this period the build-up to the invasion of Europe

began, and many materials were transported across the Atlantic. Centimetre radar made little impact upon the state of the war as far as Britain was concerned up until this point.

Centimetre research was nearly stopped during the Battle of Britain, when many people thought that all available resources should be channelled into meeting the very real and urgent threat of a German invasion. As I relate in the next chapter, centimetre research was viewed by most people as being very “pie-in the sky”, and had few friends outside the small group of those committed to its development. I believe that given the state of 1.5m AI, there was going to be pressure from the radar researchers for centimetre research given the way the war went, but that the continuation of this research during the summer of 1940 was very touch and go. If this had been cancelled, it is unlikely that it would have been resurrected. Firstly, by October 1940 mark IV AI and GCI arrived. Secondly, all the rapid progress on centimetre research occurred between June and September 1940, and without this rapid progress there would have been little weight for the researchers to justify the resources that were poured into this area subsequently.

Chapter 3: The Development of British Microwave Radar Components

3.1 Introduction

In the previous chapter I introduced the reader to the concept of radar. I covered the precursors of radar, and explained the political developments of the 1930's that led to Britain seeking a scientific solution to the problem of air defence against bombers. The Chain Home radar and the accompanying methods of directing fighter aircraft onto incoming raids were the response to this threat. Airborne radar followed out of the next worry, that of air attack by night. I explained how airborne radar was developed. I also concentrated on the nature of the problems facing the airborne team, and the characteristics that that team exhibited in dealing with these problems. Lastly, I considered some of the possible lines that radar development could have taken apart from going down the road of centimetre research.

This chapter follows the development of the key components that were used in building the first British microwave radar in August 1940. The importance of this radar to the thesis is that it was the basis of the experimental AI system out of which H₂S was developed. H₂S was the complete microwave system which the Germans captured and copied in 1943, and which I use to explore some of the issues of knowledge types and transfer that I raised in chapter 1. The story that I tell in this chapter is very similar to that told in the next. Both the development of microwave components, and then of a complete system, happened very rapidly. I wish to argue that this rapid development was due to three factors that were similar in each case.

The first factor is that the groups involved were very small, consisting of up to a dozen people, though sometimes only two or three individuals. Despite being small they were linked to other research groups, which allowed a transfer of intellectual and experiential resources. These linkages are the second factor. All the key developments were made by men who were linked in some way either to the Cavendish Laboratory of Cambridge University, or else to the General Electric Company (GEC). They were also all accomplished experimenters who had either had long experience of working in laboratories with complicated electronic or physical equipment, or else had had experience prior to their University or Research appointments that gave them practical

skills in making and operating complicated apparatus. These skills which fall into the category of embodied skills, are the final factor.

The skills that they gained can be described as embodied tacit knowledge about how to operate and construct electronic apparatus. Their practical, embodied knowledge equipped them to refine their apparatus when they were experimenting. Sometimes, they were required to make this knowledge explicit when helping a colleague. Their working arrangements facilitated this process, as they were in small groups where problems could be readily discussed, and apparatus observed by others and demonstrated to others. This environment permitted a high degree of cross-fertilisation of ideas, as I will show.

One of the main things that I wish to show is that the advancement of their ideas about what they were doing, and what they would do next, came not solely from theoretical ideas (although the theoretical nature of their work was discussed). I will indicate how their physical experience of acting through experimentation shaped their thought processes and helped them to formulate their ideas about their work. This process is described by Gooding in relation to Faraday's work with the rotation motor, when Faraday developed both his practical experimental competences and his ideas about theory *in parallel*.¹

3.2 The State of Microwave Technology Prior to the Outbreak of War

In this first section I shall look at what work existed on microwave theory and components that was potentially available to the AMRE 10cm radar team, and where this work came from. I say "potentially" quite intentionally, as some of the AMRE team members assert that they were unaware of what was available. For example, Lovell stated that:

Much later we learnt that the properties of paraboloids as aerial systems for the transmission and reception of very high frequency waves had been worked out in other countries but at that time, in 1940, we knew nothing of that work and proceeded to rediscover these properties for ourselves.²

¹ Gooding (1990), ch 6.

² Lovell (1991), p39.

Indeed, parabolic reflectors were used in France in 1935-6 to make an obstacle detector for ships.³ I intend to show that prior to the War, any knowledge that existed in Britain about microwaves was passed on through personal contact, as with Oliphant and the Varians, or was learnt through experimentation based on the limited amount of published material available. I believe that given the desultory level of knowledge about microwaves that existed in this country, the speed of developments was due to the way that the few who did have experience interacted on a close personal level with those who didn't.

3.2.1 Waveguide Research

Much work on the properties of very short radio-waves was done in the late nineteenth century (see in particular mention of Hertz's experiments in the last chapter). The early electro-magnetic wave generators were spark-gaps; such generators produce short (centimetre) length waves. However, the power given by spark-gap generators was very low which meant they were only suitable for use in laboratories. The commercial exploitation of radio that took place following Marconi's experiments used much longer waves. Interest, and research money, went into looking at the longer (tens to hundreds of metres) radio waves. Centimetre research remained low-key as there were few opportunities for using it outside the laboratory.

Very early theoretical work on the behaviour of electromagnetic waves in hollow tubes was conducted by Lord Rayleigh in 1897.⁴ In 1936, Rayleigh's work was extended by W.L.Barrow of the MIT Electrical Engineering Department and G.C.Southworth of the Bell Telephone Laboratories, both in the USA. Barrow found that a suitably matched tube⁵ to the wavelength of the wave could produce transmission of a radio wave at a strength ten times that of free space. More work was done on investigating the properties of hollow tubes as microwave carriers, up until the foundation of the

³ Guerlac (1988), ch 2.

⁴ See Guerlac (1988), ch 2.

⁵ Barrow's original waveguide was a one-foot diameter air-duct pipe. Similar inventiveness was shown by the British at AMRE, where their first waveguide was part of the plumbing for an Elsan chemical toilet. See Atkinson (1990).

Radiation Laboratory at MIT following the Tizard Mission's visit in 1940 (see Chapter 4). The Radiation Laboratory was able to draw on a local body of expertise in this area when the Americans began research into microwave radar.

Southworth's investigations began in 1931 when he started studying the properties of di-electric materials in cylinders. This led him eventually to work on the waveguide properties of hollow pipes (where the di-electric material is air). He generated the necessary wavelength by using a Barkhausen-Kurz valve, which was the usual choice of valve for generating centimetre wavelengths at this time). Importantly, he also discovered that by flaring the ends of the waveguide into a horn shape he could direct the emission into a beam. He does not appear to have envisaged many applications for his discoveries, as Guerlac reported:

Southworth was... cautious... in commenting on the possible applications of this new form of radio transmission. Its value appeared to be limited by the fact that the size of waveguide structure is proportional to the wavelength, and hence it was only suitable for the very highest frequencies. These frequencies [wavelengths] were only just being explored and as yet no suitable source of power was available on these frequencies: "The situation then is that the art at these extreme frequencies is not yet at the point which permits a satisfactory evaluation of practical use."⁶

It should be noted that there was a small but significant amount of expertise in the US about waveguides. This expertise was put to good effect in the Radiation Laboratory. The Radiation Laboratory eventually co-operated with its UK counterpart, AMRE (renamed TRE in 1942), but large-scale co-operation did not begin until after then US entered the War in December 1941. Southworth's work was not used by the scientists at AMRE. As Lovell illustrated, they proceeded to do much of their empirical investigation into waveguides for themselves when they commenced research into this area in May 1940 (see chapter 4). Therefore Southworth's work had little influence on the initial British investigations into microwave radar.

⁶ Guerlac (1988), p204. Quote taken from Southworth, G.C. (1936) "Hyperfrequency wave guides - General considerations and experimental results." in *Bell Syst. Tech. J.* **15**, pp284-309.

3.2.2 The Klystron

One of the important features of microwaves that made them interesting for research was how they behaved in a resonant cavity; which is what, in effect, a waveguide is. The behaviour of resonant cavities in connection with their use in electronic valves was first investigated by William Hansen of Stanford University in California⁷. He was looking for a low-cost way to design a high-energy valve for x-ray research, which he was doing under the tutelage of David Webster. When Hansen was an undergraduate student at Stanford his potential was recognised by the staff and he was “groomed” to become a member of the faculty. He then went off and did his graduate work at MIT in x-ray crystallography. Ginzton, a contemporary of Hansen’s, described his abilities as a researcher:

At graduate school, his childhood training with tools and mechanics blossomed and he became an excellent machinist. Later these mechanical skills became important in the klystron project as he was able to identify Russ[ell Varian]’s more practical ideas and to help Sig[urd Varian] design equipment.⁸

Prior to the Second World War it was far more common for scientists to build their own experimental apparatus. This meant that they often learned practical skills, such as those that Ginzton listed above. Hansen would have got a feeling for the physical possibilities of the materials that he worked with, both in terms of constructing apparatus and in terms of the physics he performed with it. This physical knowledge would have helped him to refine his colleague’s ideas into practical lines of experiment.

Hansen’s ideas for using resonant cavities came from work he did as a graduate student on examining oscillations within a sphere. Hansen discussed his ideas for using resonant cavities with Russell Varian, who was another student of Webster’s.⁹ The trouble with Hansen’s ideas for generating high-energy x-rays were the limitations of the contemporary technology. Building a device with sufficient power would mean him constructing equipment of 15 feet in diameter. Hansen put a proposal before the Head of

⁷ The reasons that Stanford came to be the scene of this research are explored more fully in Norbert & Seidel (1994).

⁸ Ginzton (1977), p717.

⁹ Norbert & Seidel (1994), p206.

Faculty, but as funds were tight and such a large piece of apparatus would eat up a not insignificant proportion of the available research budget for the department, he was told to go away and make a mathematical analysis of how his device would behave. Ginzton added:

As surprising as it may seem now, the idea of a microwave cavity resonator was not yet known and Hansen was exploring the utility of the idea both intuitively and by complex mathematical analysis employing boundary value approaches.¹⁰

His time was not wasted, though, because this work gave him sufficient ammunition to persuade his colleagues that the project was viable. He constructed a working model in February 1936, but:

...while simple enough in theory, and prototype, in practice it was another matter entirely. Over the next year Hansen built his “Big Rhumbatron” as he called it, but major problems with the triodes, cooling system, and blocking condensers limited efficiency.¹¹

Hansen’s mathematical investigations were useful to him in helping to formulate ideas as to how to build his device and also, of course, in persuading those in control of the purse-strings to give him the money to build it. He used mathematical models in conjunction with his own experience in order to design the equipment that he built. However, his theoretical work was no substitute for practice. He had to perform experiments with apparatus, and go through the trial-and-error stages of building up “know-how”. It was only through engaging with his apparatus that he was able to learn how to make it work.

Hansen’s impetus for developing the rhumbatron was as an electron accelerator for x-ray experiments. However, the impetus for the development of the resonant cavity principle that resulted in the klystron, which was also Hansen’s idea, came from a different direction. Whilst Hansen was working on the rhumbatron, his erstwhile colleague Russell Varian moved to the Farnsworth Television Laboratory, and then on to

¹⁰ Ginzton (1977), p717.

¹¹ Norbert & Seidel (1994), p207.

his own laboratory. Russell's brother Sigurd was an airline pilot with Pan Am, and the two often discussed ways of becoming rich through having an idea for an invention. Sigurd's idea was for a means of detecting aircraft through cloud¹². Russell believed that any such device would need to operate on centimetric wavelengths, and that Hansen's rhumbatron may well be the source of such high-energy, high-frequency power. He discussed the possibilities with Hansen, and when Sigurd took leave of absence from Pan Am the three set about the problem in Spring 1937.¹³

Hansen was concerned about the performance of the triodes in his rhumbatron, and looked for a different valve that might alleviate some of those problems. He discussed the problems with the Varian brothers and recorded these discussions in his laboratory notebooks. A combination of discussion, diagram-drawing, experience with the rhumbatron and consideration of the problem led Hansen to devise the "bunching principle". He imagined speeding up and slowing down the electrons travelling across the evacuated space in the valve so that, rather than travelling in a uniform stream, they travelled in groups (rather like firing distinct bursts from a machine gun). This bunching would be at a frequency that could be controlled, and would be of the right order to produce microwaves. These ideas led to them constructing a valve that incorporated the bunching principle, and putting it to the test.

Their first tests gave very encouraging results, with the new valve (which they named the klystron) giving off 13cm radiation at sufficient power to be detected around the room. These initial results were enthusiastically greeted and gave grounds for considerable optimism, as Russell's wife wrote: "This, of course, means that the victory is practically won and it is just a case of time before they get the other little items straightened out. Dr Webster was so thrilled that he invited the physics department and myself over to his house for beers".¹⁴ However Mrs Varian's optimism was ill-founded, as:

[I]t required four years of further development before the klystron became a production-line item. The next four years proved to be a trying time for the Stanford

¹² This was at the time of the Japanese invasion of China, and of the Spanish Civil War when the effect of airpower was first realised.

¹³ Guerlac (1988), p196.

¹⁴ Winnie Varian to Allie and Weonah, August 31, 1937, Varian Papers, University Archives, Stanford University, quoted in Norbert & Seidel (1994), p209.

team, while Stanford patented the klystron immediately, they entered into a development contract with the Sperry Gyroscope company without realising the implications of development. The tension between industrial proprietary rights and academics' propensity to open publication strained relations between the two groups.¹⁵

Hansen had devised a useful principle, but a lot of work had to be done before this principle could be incorporated into a production valve. Hansen and his team had to spend time experimenting in order to learn how to make their klystron work as they wished it. When they had done this, and had developed the requisite tacit knowledge, their embodied skill could be embedded into an engineered valve. However, the difficulty which they had in reaching this stage meant that the production valve took four years of development.

This recalcitrance is important to note, as the klystron is of major interest to the story of British microwave radar development as I describe in the next chapter. The other important point is that Oliphant¹⁶, who headed the Birmingham team that developed the cavity magnetron, went to Stanford to see the klystron:

Now Oliphant, who had been informed at the close of 1938, by Sir Henry Tizard and Professor J.D.Cockcroft, about radar progress and its problems, had visited Stanford University, among other places in the U.S.A., in January 1939 and had acquired much information on the recent klystron work of the Varian brothers.¹⁷

When the British attempts to build a working klystron are related in the next chapter, it will be seen that they also had quite considerable difficulties in getting it to work. This was at a time when the klystron had been in existence for three years. These difficulties were also experienced despite Oliphant having had personal contact with the originators of the valve. Oliphant certainly had personal contact with Hansen and the Varians, and was shown their klystron. This is something that was related by Burcham & Shearman:

¹⁵ Norbert & Seidel (1994), p209.

¹⁶ Oliphant was originally an Australian, but he came to Britain in 1927 to work at the Cavendish laboratory. In 1937 he moved to take up the Poynting chair of Physics at Birmingham. He transformed what had been up until then a relatively small and quiet department, orienting it towards nuclear research and getting the Nuffield Laboratory built. Oliphant (1990), Burcham & Shearman (1990).

¹⁷ Burns (1988b), p269.

[H]igh powers (though not yet at short wavelengths) were... needed in linear and cyclic accelerators for nuclear physics and early in 1939 Oliphant visited the United States to see Ernest Lawrence, the cyclotron physicist, in California and to learn about the new klystron generator of power at very short wavelengths which had been developed by the Varian brothers (1939) at Stanford. He brought back with him the technical information necessary for the construction of one of these at the Birmingham laboratory.¹⁸

When Oliphant visited Stanford, the klystron was not *stable*. By this I mean that Hansen and the Varians had not yet acquired the skill to operate their klystron - they could not produce the required effect *every time*. By his visited he acquired the algorithmic information required for building a klystron, and he presumably had Hansen's embodied tacit knowledge about klystron operation made explicit to him by his personal contact. However, as Hansen did not possess the experimental skill to make it work every time (because he had still to learn how to do this) Oliphant would have the same trouble as Hansen, even if he had had *complete* knowledge transfer about the klystron. This was why the British had trouble with their own klystron in 1940.

3.3 The Origins of British Interest in Microwaves

Microwave radar was developed in Britain primarily as a response to the perceived shortcomings of AI marks I - IV. Bowen, and others, had identified the main problem as being a limited maximum range (the so-called "minimum range problem" was solved by the Mark IV variant). Because these versions of AI used 1.5m waves, the set "floodlit" rather than producing a narrow beam, and the maximum range of the set was limited by the aircraft's height above ground. The ground would always produce a large return on the screen that would swamp any signal within the theoretical maximum range of the system, but further away than the ground (see chapter 2). The Air Ministry team that constructed the first microwave radar were able to draw on a base of experience, particularly in the form of manufacturing expertise and skill, gathered from several years' interest in the potential of microwaves by both the Military and industry. It was the

¹⁸ Burcham & Shearman (1990), p8.

eventual coalescing of this interest into a unity of purpose, that of developing high-power sources for centimetric waves, that I believe enabled the remarkable rapidity with which this project bore fruit in the shape of the cavity magnetron.

More specifically, I will argue that the development of the cavity magnetron was the due to the combination of four lines of research and experience, namely:

- (i) Hansen's resonator principle from the klystron;
- (ii) Randall & Boot's experience of valve manufacture and their knowledge of the resonator principle, coupled with their innovative combination of this new principle with the magnetron valve-geometry;
- (iii) Gutton's work on split-anode magnetron improvement; and
- (iv) the refinements made by Megaw to unite these three strands into the pre-production, engineered E1189 cavity magnetron.

In each case the experimenters had considerable "hands-on" practical experience and background knowledge of their field, but they combined this knowledge with an ability to push their work into uncharted areas where their only guidance was their "intuition". I would argue that this was due to their good understanding of the physical possibilities of apparatus from their considerable practical skills. These skills gave them a "feel" for what was possible. They had unarticulable "know-how" about how to get an experiment to work, similar to Hansen and his klystron.¹⁹

The British Government's research establishment's interest in centimetres began when the Co-ordination of Valve Development (CVD) Committee placed a contract for the development of high-power centimetric valves with Birmingham, Oxford and Bristol Universities in Autumn 1939. This was shortly after the outbreak of the Second World War, but interest in developing high-power centimetre valves had been developing over the previous two years. To explain where and why this interest originated, it is necessary to retrace a few steps to bring together several threads.

CVD was formed in early 1939 in an attempt to formalise an already existing arrangement between the three Military Services and their various commercial valve

¹⁹ See also, for example, how Morpurgo and team proceeded to investigate the existence of Quarks by using a charged oil-drop experiment. Gooding (1990), chapter 8.

suppliers. The committee was supposed to cut through the secrecy surrounding radar development, which was hampering co-operation and standardisation between these organisations and producing a duplication of effort not deemed to be in the national interest. The Air Ministry's needs were being met by the work done by its establishment at Bawdsey, which had accrued four years' worth of experience in radar development. Bawdsey already had dealings with suppliers for components, which I have covered in the previous chapter. The Admiralty relied on its own Signal School for valve development (also mentioned in chapter 2; the Signal School supplied the valves for the original radar experiments), but:

Although Admiralty and Signal School were aware of the work at Bawdsey from the beginning, it was considered that the design and construction of ship-borne installations was sufficiently different from land-based or aircraft equipment to justify an independent approach to their development.²⁰

One interpretation of this state of affairs could be simply the Senior Service wishing to preserve its "empire" intact. Both the Navy and the Army had fought to suppress the retention of the RAF as a separate service after the First World War, so this is a possibility. Nevertheless, the Navy's Signal School too was a repository of considerable skill in valve design and manufacture.²¹

The Admiralty developed its own 7m wavelength ship-borne radar designed to detect aircraft, Type 79, which entered service in October 1938. Naval research was independent to that of the other two Services, and there were only informal contacts between researchers in the different camps. It is not known how much they shared with each other.²² The valves for this equipment were made by engineers in the Signal School, and the group developing these valves was led by J.F.Coales. The group did some calculations in early 1938 that led them to believe that a wavelength of 50cm would be most suited to surface-vessel detection. The Admiralty also had a contract with GEC for the development of very short wave valves for communication purposes. Part of this contract was for a propagation study, done by E.C.S.Megaw, who would later play an

²⁰ Callick (1990), p2.

²¹ Further details can be found in Foley (1991).

²² Co-operation between the services was negligible in Germany, as I describe in chapters 6 and 7.

important part in refining the newly developed cavity magnetron. It was while Coales was involved in discussion with GEC that the decision to form the CVD Committee came about. Coales was unable to discuss the usage (radar) to which the valves would be put with the GEC staff, as none of them had the security clearance necessary to know about this top-secret application. Coales mentioned the problem to C.S.Wright, the Admiralty's Director of Scientific Research (DSR). Wright gave approval for certain individuals at GEC to be informed about radar, and the arrangement was formalised in the form of the CVD Committee a few weeks later. This committee then met at regular intervals thereafter, with Watson-Watt coming to the second meeting on 14th February 1939.²³

The establishment of CVD is very important for several reasons. Its primary purpose was to co-ordinate valve development and manufacture between the Services' research establishments and the several manufacturers (though primarily GEC) engaged in producing valves for them. As such it provided a level of co-operation between all the various parties interested in radar that didn't exist in any other country at this time, and in the case of Germany, ever. For the purposes of my interpretation of events, I believe it allowed the easy transmission of knowledge about techniques, processes and equipment at the opening phase of the development of high-power centimetric valves. In terms of skill and practice, it provided the forum that enabled people with a problem to become aware of someone with perhaps the necessary skills to help solve it. CVD was very important because it was the means by which personnel could be introduced to each other to allow the transfer of valve- and radar-building skill in the form of ideas and practical competences.

3.3.1 The Split-Anode Magnetron: GEC's and SFR's Contributions

The split-anode magnetron was invented by A.W.Hull²⁴ of the American GEC in 1921. It was a glass-envelope valve that consisted of a cylindrical anode surrounding a central cathode, with a magnetic field applied along the axis of the cathode/anode

²³ Callick (1990), pp2-3. Details of what passed at the CVD meetings can be found here.

²⁴ See Hull (1921a, b).

combination. Because of its geometry, the valve oscillated in a mode that produced waves much shorter than those of standard triode-type valves. In the 1920s and 1930s the valve was refined²⁵ by researchers in various electrical companies. During the 1920's several researchers in different countries conducted experiments with the valve, but its power was low and it remained at best a laboratory oddity. At the beginning of the 1930's interest was revived in centimetric waves in Europe, the U.S. and Japan when it was realised they would be suitable for directional communications purposes.

In the 1930's the largest manufacturer of valves in Britain was GEC, which had a research laboratory in Wembley. From 1932 a small group at this laboratory under E.C.S. Megaw were attracted by the commercial opportunities that they thought were possible with very short waves. They began propagation studies on wavelengths below 60 cm at this time. This work included investigating the properties of the split-anode magnetron. In 1933 Megaw published a paper detailing his investigations into the phenomenon of "back-propagation", whereby electrons returned to the cathode causing further heating and emission. He also mentioned that back-propagation may be useful in producing very short wavelengths.²⁶

The Philips company of Holland was also interested in magnetrons, and one of their engineers, K. Posthumus, did some experimentation during 1934-5. Splitting the magnetron anode into two segments had been investigated in 1924, by Habann. Posthumus took the idea one step further, using four segment anodes, and found that increasing the number of anode segments gave a corresponding decrease in the magnetic field strength and the accelerating voltage required for a given power output. He explained these improvements in efficiency in terms of a theory of a rotating electron cloud, and his paper influenced Randall and Boot's thinking in 1939.

Megaw studied Posthumus' work closely and conducted some further experiments. One aspect that followed on from Posthumus' paper was the potential of back-bombardment, and Megaw concluded that "secondary emission could provide a major fraction of the operating current".²⁷

One of the other major workers on the split-anode magnetron was Dr Henri Gutton of the French company SFR. He was the head of a team who built an obstacle-detector

²⁵ For details, see Guerlac (1988), pp187-92, and Swords (1986), pp259-263.

²⁶ Callick (1990), p59.

²⁷ Callick (1990), p60.

for ships using a wavelength of 16cm. It was fitted to the liner *Normandie* in summer 1935, but bad weather and damaged valves prevented him from making any tests and the equipment was abandoned. Gutton used Barkhausen-Kurtz valves for this work, that operated on a wavelength of 16cm, and his first obstacle detector wasn't pulsed but utilised the continuous wave method. Whilst considering the problems of his detector after the failed *Normandie* trials, he decided to change to pulse operation and use split-anode magnetron valves. He conducted experiments in 1936 and 1937 and read of Posthumus' work, and in 1937 patented an 8-segment²⁸ valve which gave 10 watts of power on 16cm. Gutton was visited in 1939 by Megaw to discuss the work he had done on magnetrons, which I will cover in the next section.²⁹ Importantly, this was an opportunity for these two engineers with magnetron-building skill to exchange ideas and techniques about how to improve the valves, something that Megaw would put to use with great effect when he refined the cavity magnetron.

3.3.2 The Cavity Magnetron: Development at Birmingham and GEC

As these events took place in various parts of Europe, the network that formed the British Scientific Establishment, civil, government and military, set in motion the train of events that led to the engineered cavity magnetron. This network was one that had grown over a number of years since the formation of the CSSAD in 1935. As the number of radar workers increased, so new people were let into the radar secret. It started off with the members of this committee, and gradually expanded to include men like Watson-Watt, Cockcroft, Rutherford and the like. British science was a lot smaller than it now is, as there were far fewer laboratories. However, there were some linkages between personnel that were far more important than others.

The Cavendish Laboratory and the Athenaeum Club in London had been two important nodes in the network of Establishment linkages at the highest levels of British science for the previous two decades. Many members of the British scientific elite, some of whom were members of the committees engaged in deciding the future of British air

²⁸ This refinement gave shorter wavelengths with the same valve geometry.

²⁹ Molyneux-Berry (1988).

defence, were members of the Athenaeum, and many of these had at some period trained or worked at Cambridge University's Cavendish Laboratory for Physics. The connections between the Cavendish and the Bawdsey radar research establishment were widened during the Spring and Summer of 1938, and again in the summer of 1939 when, because of the impending war, many University scientists were shown the hitherto secret Chain Home stations and introduced to radar.

In the Spring of 1938 Sir Henry Tizard lunched at the Athenaeum with Cockcroft of the Cavendish to explain the nature of the secret radar work going on at Bawdsey, together with some of the problems the people there were experiencing. Tizard realised that if war broke out, there would have to be a large expansion of research and development into radar applications, so he set about making contacts with personnel that he thought would be useful to recruit or help recruit others. Tizard already had links into the world of University science firstly through knowing the other members of the CSSAD (the "Tizard Committee"), and secondly through being the Rector of Imperial College.

Tizard and Cockcroft's meeting sent ripples across the pool of British University science. In the Autumn of 1938 Bragg, head of the Cavendish and superior to Cockcroft, wrote to Sir Frank Smith, head of the Department of Scientific and Industrial Research (DSIR) asking for the "important leaders of research in many Universities" to be informed of radar problems. This action would create a wider network of people who knew about and could contribute to radar research. He forwarded a copy of his letter to Tizard, who wrote back to Bragg saying that:

You say in your letter 'sometimes a problem can be put to outside people which is of such a general kind that no secret is given away by formulating it.' This is just what I am proposing to do if I come up to Cambridge to have an informal talk with the senior people there. What I thought of doing was to state as clearly as I can what the main problems are, without necessarily disclosing all the ways in which they are being attacked. It can do no harm to get people thinking on the right lines.³⁰

Bragg's contact with Tizard led to the drawing up of a list of people who were deemed by them to be suitable for initiation into the radar secret. This list included men at most

³⁰ Letter, Tizard to Bragg, quoted in Clarke (1965), pp171-2.

of the major University physics departments of the time, namely those of Oxford, Cambridge, London, Bristol, Manchester and Birmingham. Importantly many of the senior members of these various laboratories had either spent time working at the Cavendish, or had been students there. They were at least familiar with each other through this link, if not always on the level of personal friendship. This knowledge greatly eased co-operation in some ways, although occasionally it could also lead to friction (as in the case of Lewis and Dee, as I relate in the next chapter). Inclusion on the list was by personal recommendation of people whom Tizard and Cockcroft trusted. Inevitably this meant that they relied on their friends in departments at other Universities. It also meant that non-physicists were included (as in the case of Alan Hodgkin, as I discuss in the next chapter).

Over the summer of 1939, the scientists that were on the lists drawn up by Tizard were taken round Chain Home stations and told of the advances that had occurred in detecting aircraft through using radiowaves, and also of the problems still being faced, namely those in AI. It was in this way that Oliphant was introduced to radar during the Summer of 1939, as he recalled in 1990:

Tizard was undoubtedly the main influence which induced us to abandon, temporarily, the work going on in Birmingham on nuclear energy, in order to devote ourselves to microwave radar. Our experience at the 'chain' station, where we were introduced to the mysteries of RDF, and discussion with Cockcroft, Bowen and Rowe, convinced us of the advantages which might be expected from the development of generators and detectors of radio frequencies far higher than those then in use. It was Tizard who persuaded me that a contribution in the field could be of immediate value in the war with Germany, and I have no doubt that it was Tizard who persuaded Charles Wright to propose to me a practical way in which we could work on these problems under Admiralty auspices.³¹

In the Autumn of 1939, CVD placed contracts with Birmingham, Oxford and Bristol for the development of these valves, and Signal School remained an interested party in what went on. Oliphant remembered receiving notification in the form of sealed written orders:

³¹ Letter, Oliphant to Clarke, quoted in Clarke (1965), p203.

Immediately after the outbreak of war, I was surprised to receive an envelope, inside which was another, marked TOP SECRET in red letters. It was from the Admiralty, asking that I get together a small team to try and develop a generator of radio waves of wavelength about 10 centimetres. The University agreed that I should undertake secret war-work without demanding to know what it was, and arrangements were made with the Admiralty to meet any expenditures involved. Jim Sayers, who had worked with Appleton [who worked on ionospheric detection in the 1920s - see chapter 2 - and was also a member of the Cavendish], was allotted to us; Randall, who had been working in the Lab on phosphorescence, became part of the team, as did Nimmo, a New Zealander. A research student, Boot, joined Randall, and Titterton, who had taken his PhD and had been teaching, returned to the Lab to provide pulsed power supplies. We knew of the development of the klystron, at Stanford, which produced small outputs in the desired frequency range, so Sayers undertook to try to push far greater electron currents through the coupled resonators, to develop higher output power.³²

This quote indicates that Oliphant was in the possession of an important piece of knowledge - that it was possible to generate high-frequency waves with a klystron, albeit at low powers, even if he didn't know how just yet.

Oliphant's team were all physicists who had no direct experience of radio engineering, apart from Sayers who had worked on ionospheric research at the Cavendish. It is interesting that Boot was described as "a research student who had already shown great aptitude in making things work"³³. So, despite not having any direct experience of radio engineering, these men had acquired practical skill in working with electronic components and of the physical ideas behind them. Burns wrote that:

Oliphant felt that this ignorance was an advantage and not a handicap, for it meant that his staff would not be inhibited by prior learning and experience about what could and could not be done: they would be able to work from first principles.³⁴

According to Shearman & Land, Randall "was a former colleague of E.C.S. Megaw at the Research laboratories of the GEC"³⁵. This contradicts the assertion that they would

³² Oliphant (1990), pp7-8.

³³ Shearman & Land (1985).

³⁴ Burns (1988b), p269.

³⁵ Shearman & Land (1985).

not be inhibited by prior experience, but lends weight to the idea that experience and contact are essential components of “serendipity”. Randall’s prior career of working with electronic valves at GEC would have given him the sort of acquired tacit knowledge of what could and couldn’t be done in the realm of valves that would be useful to him in working on this problem.

Oliphant divided his team up to look at various aspects of the centimetre problem. They decided to start with the hitherto accepted methods of generating centimetre waves: the klystron and the Barkhausen-Kurtz valve. Oliphant and Sayers looked at the possibilities of improving the klystron, Moon and Nimmo at how it could be used to either generate or receive³⁶ radio waves in a radar system, Titterton undertook building a 50cm system using GEC’s “micropup” valves, already used in 1.5m AI, and Randall and Boot were given the Barkhausen-Kurtz oscillator to look at.

Randall’s view of this in 1946 was that:

At first Boot and myself spent a few weeks studying the Barkhausen-Kurtz tube as a detector device; the greater number of workers in the laboratory, however, were concerned with the klystron, both as an oscillator and as an amplifier, and we were naturally interested in the whole field.³⁷

The klystron was the subject of discussion in departmental colloquia earlier that year when the Varians’ papers were published, which is where Randall and Boot were introduced to the idea of resonant cavities. However their allotted task of investigating the Barkhausen-Kurtz valve was proving “unpromising”, and Randall and Boot became “disenchanted with this task”.³⁸ As they indicated, they started to put their minds to the problem of a high-power transmitting valve. They added that:

[A]t the risk of incurring some unpopularity from our fellow workers, we concentrated our thoughts on how we could combine the advantages of the klystron with the more favourable geometry of the magnetron.³⁹

³⁶ The klystron would eventually become used for this purpose in centimetre AI in the form of the reflex klystron, or “soft-Sutton” valve..

³⁷ Randall (1946), p248.

³⁸ Boot & Randall (1976), p724.

³⁹ Boot & Randall (1976), p724.

Randall and Boot were unconvinced by the klystron. One of the main problems with operating it was getting the electron beam focused enough. They were also only operating it continuously (not pulsing it), and they “believed that its pulsed peak power output would not greatly exceed its CW power.”⁴⁰ This sort of belief about how relatively novel equipment will or won’t operate is characteristic of how skilled experimenters are able to direct their thoughts and future experimental directions into fruitful areas. It comes from having experience of working with apparatus; of having a “feel” for the possible. Gooding has analysed this procedure in relation to Morpurgo’s Quark experimentation in the 1960’s. Morpurgo’s team conducted a number of preliminary experiments to get experience of working with their apparatus. They developed their apparatus by using “available precedents”, which led them to choose an oil-drop apparatus similar to Milikan’s. However, their preliminary experimentation with this apparatus ‘ “destabilised” the model which embodied the team’s understanding of possible instruments.’⁴¹ This is the same process that Randall and Boot underwent with their attitude to using the klystron. Preliminary experimentation with it led them to reject it as unsuitable.

In November 1939 they came up with the idea that led to the construction of their first cavity magnetron. The circumstances can be judged in hindsight as extremely fortuitous now it is known that their idea was a resounding success, but of course at the time they had no idea that their thoughts would lead so quickly to a solution of their problem.

In their 1985 article, Shearman and Land introduced some parts of the thinking that led to Randall and Boot’s cavity magnetron idea. Shortly before war broke out Randall went on holiday with his family to Aberystwyth. Whilst there he bought an English translation of Hertz’s *Electric Waves*, which contained a description of how Hertz detected radio waves using a wire loop receiver. This idea of a loop of wire would influence him in the design of a suitable cavity. Furthermore:

⁴⁰ The two were introduced to the principles of radar operation at Ventnor Chain Station during the summer of 1939. Whilst there, they were able, as were all the physicists, to study the circuit diagrams of the equipment, and even to suggest modifications “which of course always made its performance worse.” Boot & Randall (1976), p724.

⁴¹ Gooding (1992), p83.

An element in their thinking may have been the seminal suggestion to Oliphant on a visit to the Admiralty Station at Haslemere that the combination of the split-anode magnetron with resonators after the pattern of the klystron described by the Varian brothers may be a way forward to shorter wavelength operation. Randall was a former colleague of E.C.S. Megaw at the Research Laboratories of the GEC and was aware of the limitations of the pre-war magnetrons of two and four anode construction and external Lecher wire resonators.⁴²

This passage tells us of the level of knowledge about split-anode magnetron valves that Randall possessed. This knowledge would help him and Boot, I believe, to make their next step.

Randall and Boot were equipped well for their next leap of thought. They understood about conventional magnetrons, they also understood something of the new field of resonant cavities, and they were disillusioned with the research they were following on the Barkhausen-Kurtz valve. They had studied papers by the Varians on their klystron work, and had *seen* papers on conventional glass-envelope magnetrons, but “[f]ortunately [they] did not have the time to survey all the published papers on magnetrons or [they] would have become completely confused by the multiplicity of theories of operation.”⁴³ They were also fortunate, as they have said, in being part of a dynamic laboratory well equipped with a workshop and skilled technicians. The Varians’ paper had also given them some insights into how to construct cavity resonators to the best effect, namely that:

On any given mode [of oscillation] the frequency is largely, if not entirely, dependent on the dimensions of the resonator. When such resonators are constructed of copper, three important features are evident:- (i) low h.f. losses; (ii) wave-length stability; (iii) potential capability of large heat dissipation.⁴⁴

Their thoughts now turned as to how to combine the geometry of the magnetron, which has a central cathode surrounded by a cylindrical anode (the magnetic field is applied along the axis of the anode/cathode combination), with some sort of resonator. Unfortunately the resonator types associated with the klystron, cubes and spheres, did

⁴² Shearman & Land (1985).

⁴³ Boot & Randall (1976), p724.

⁴⁴ Randall (1946), p248.

not suit the shape of the magnetron. Randall's rereading of Hertz came into effect here, as:

The only other types of short-wave resonator circuit with which we were familiar were Hertz's original loop-wire resonator and a short-circuited quarter-wave line.⁴⁵

[B]ut this was not a cavity. A cylindrical extension of Hertz's wire loop became a cylinder with a slot along a generator and it occurred to us that a number of these would fit around the slotted anode of the magnetron we were trying to invent. Also, it would be very simple to build in the laboratory workshop. Only drilling, turning and slotting would be needed. It also occurred to us that a series of 1/4 wave radial slots would also serve as resonators as they were 3-dimensional versions of a lecher line.⁴⁶

(See over for illustration). The remaining matter for them to fix was how big they should make the dimensions of the resonating cylinders. In a book published in 1902, H.M. Macdonald calculated that the resonant wavelength of Hertz's loop was covered by the formula $\lambda = 7.94d$, where d was the diameter of the loop. As Randall and Boot were trying to produce 10cm radiation, they fixed d at 12mm. The depth of the anode block was restricted by the dimensions of the laboratory's electromagnet to 4cm.

In December 1939 Randall & Boot's resonator anode design was machined in the laboratory workshop, the technician being Tom Gardiner. They had heard of the oxide cathodes developed by Henri Gutton in France⁴⁷ through Randall's connection with Megaw at GEC (see previous section), but they decided against using one as they thought it would introduce an extra complication, and unknown quantity, into their set-up. Instead they used the standard, and better-understood, tungsten cathode. The whole valve was continuously evacuated with the glass to metal seals being made with sealing wax. Cooling was supplied by water to the copper endplates, which had holes drilled in them to allow easy replacement of the cathode should it burn out. This happened frequently (See over for illustration of Anode Block).

There is a difference between constructing a new apparatus or equipment and actually making it work. By analysing Pickering's account of Morpurgo's hunt for the

⁴⁵ Randall (1946), p249.

⁴⁶ Boot & Randall (1976), pp724-5.

⁴⁷ At least this is what Randall said. Callick (1990), p62, said "Gutton's results with the oxide cathode were not known to British workers because of the ban on communications imposed by French security following the outbreak of war."

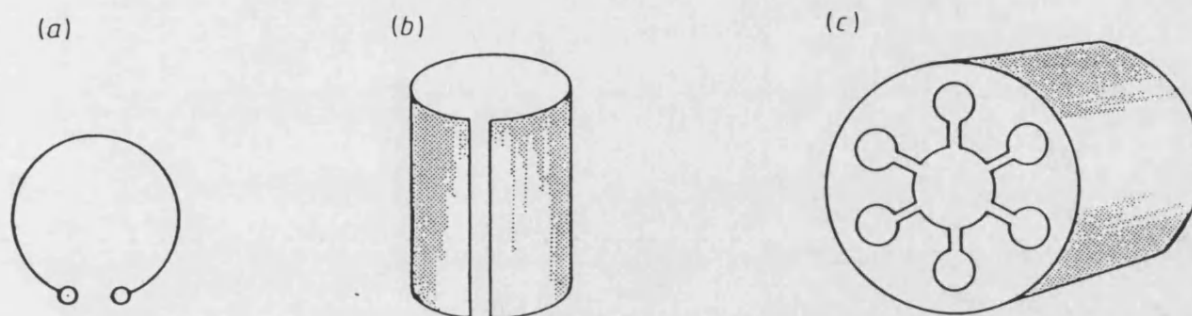


Figure 3.1 The genesis of the anode block. Randall and Boot's thoughts manipulated a hertzian loop into a cylinder with a slot. This cylinder was then joined onto another cylinder, along with five others, to form the anode block of the cavity magnetron. From Bowen (1987), p147.

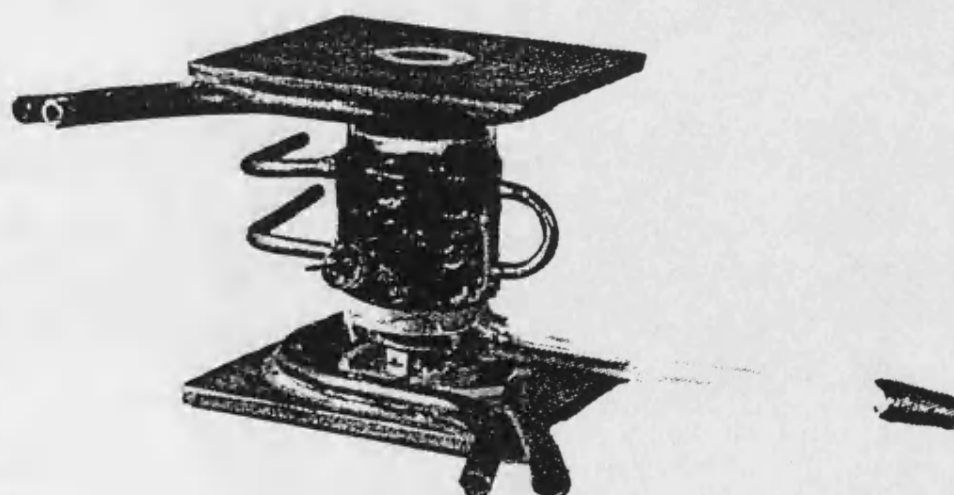
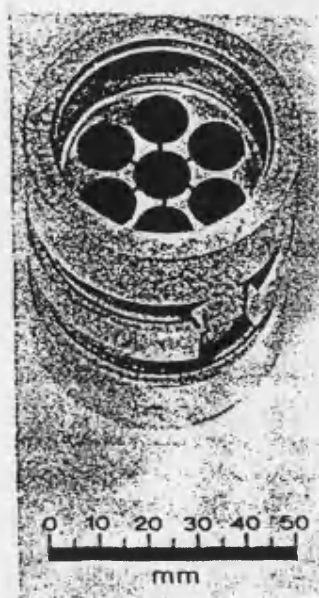


Figure 3.2 The anode block and the prototype cavity magnetron. The cavity magnetron (right) was Randall and Boot's experimental prototype. From Randall (1946b), p306.

Quark, Gooding shows how Morpurgo and his team had to modify their concepts and models in order to get a working device. I have already mentioned how their preliminary experimentation led them to reject the Milikan oil-drop apparatus. For example, they now decided to use a magnetic levitation method, but when they tried it they found their apparatus would not fit between the poles of the largest electromagnet they had available. “They [then] use[d] classical field theory to redesign the magnet poles.” Therefore it was only through actually trying to get the apparatus to work that they discovered this problem. This was the same problem that Randall and Boot faced with their apparatus, and many other experimenters have faced.⁴⁸

Thus there was a gap of some two months between the construction of the valve and the first time it produced oscillations. This is fully commensurate with the idea that they had to build up their skill in interacting with the new valve and the associated set-up. In the 1946 paper Randall was quite coy about this, saying simply that “[f]or various reasons the first trials were delayed”.⁴⁹ For an explanation we have to look further forward in time and distance from events to Randall & Boot’s 1976 review of their work, where they said that:

Although the initial tests were to be CW for simplicity, a suitable power supply posed great difficulty because all the high voltage rectifiers were in use for the klystron experiments so it was necessary to make two continuously pumped thermionic diode rectifiers. The complete set up... now contained three mercury diffusion pumps [the third being for the cavity magnetron itself].⁵⁰

Their first experimental set up was a remarkable achievement, and a testament to both the practical skill of the laboratory technicians and Boot, who built it.⁵¹ However, as I have noted above it is rare for innovative equipment to work first time, or to work as expected, and we have confirmation of both of these phenomena in Randall and Boot’s descriptions of their apparatus:

⁴⁸ Gooding (1992), p85.

⁴⁹ Randall (1946), p249.

⁵⁰ Boot & Randall (1976), p725.

⁵¹ Boot’s prowess was acknowledged by Sayers: “the superb experimental ability of Harry Boot”, and Moon: “Born and educated in Birmingham with no silver spoon in his mouth but with an engineer’s instinct in his brain and (I suspect) a lathe in his father’s garage... There is no need to enlarge upon Harry’s famous skill.” Sayers (1990), p12; Moon (1990), p14.

Powerful oscillations were obtained on the morning when all this equipment *worked simultaneously* [my italics] and that was 21 February 1940.

and...

There was also the day when no power could be obtained *until it was found out* [my italics] that the output coupling loop had burned itself out.⁵²

When all the equipment did finally work together, on February 21st, there was great excitement in the laboratory. The output lead gave off great power, which was initially evident from what they observed when they turned the on apparatus:

The amount of power produced was, for the time, capable of quite spectacular effects. It was uncomfortably hot to hold the hand near the output lead of the magnetron and a small arc sprayed off into the surrounding air⁵³. An attempt to estimate the power output was made by the very crude method of burning out successively larger 6V filament lamps and it was found that the low pressure neon-lighting tubes, (about 1m long by 37mm diameter) could be lit to a brilliancy corresponding to a power consumption of 400w, [circa 6A, 70V at 50Hz]. That the output had proved rather more than we had anticipated originally, is shown in laboratory notebooks of attempts to prove that the wavelength was not centimetric, but metric. The wavelength was measured however the next day by means of a pair of Lecher wires about 3m long, and it was shown to be 9.8cm.⁵⁴

It is interesting to note the important point that Randall and Boot initially disbelieved their own results. They had succeeded in creating something that produced an effect wildly different from what they expected, or what anyone believed possible.

After a few more days of experimentation, their remarkable results were communicated to Charles Wright at CVD on 27th February, who discussed them with others including W.B.Lewis, Deputy Superintendent of AMRE. At the CVD meeting of

⁵² Boot & Randall (1976), p725.

⁵³ Compare this to Batt's description of the klystron at Worth Matravers later that year (chapter 4).

⁵⁴ Quoted from Randall, J.A. and Boot, H.A.H. (1945) *The development of the multi-resonator magnetron at the University of Birmingham, (1939-1945)*, in Burns (1988b).

5th April 1940 Lewis initiated a discussion on the advances made at Birmingham and arrangements were made for Megaw of GEC to visit Birmingham in order to begin producing a fully engineered production valve. Megaw went to Birmingham on April 10th. The arrangement between CVD, GEC and Birmingham was formalised by correspondence between Oliphant and Sir Clifford Paterson of GEC. It was agreed by them that GEC would make sealed-off versions and help Birmingham with techniques and materials. S.M.Duke, a GEC technician, was seconded to the Birmingham laboratory in June.⁵⁵

Megaw was GEC's expert on magnetron construction, and as I mentioned in the previous section, he made a private visit to SFR (France) in June 1939, where he was shown the newly developed oxide-coated cathode employed by them in a four segment split-anode magnetron. In early May 1940 Megaw was visited by a member of SFR, Dr Maurice Ponte, who brought improved versions of the earlier valve that Megaw saw the previous year. When Megaw went to Birmingham he suggested that they use an oxide-coated cathode to improve the pulsed peak-power output. He also informed them of the revolutionary gold-sealing technique developed by D.A.Boyland for GEC, whose use in the cavity magnetron was suggested by Le Rossignol⁵⁶. This technique was suitable for sealing copper to copper, which the cavity magnetron case and endplates were made from, and involved using gold wire heated to 500°C under pressure. It also resulted in neater seals than using a brazing method (which the Germans used in their copy of the magnetron - see chapter 7).

The first valve design produced by Megaw, designated the E1188, had a tungsten cathode. This meant that it was suitable only for CW operation because the cathode could not cope with the higher powers used in pulsed operation. A CW high-power centimetre valve was what the Admiralty required for their communications project, so Megaw completed the design. A benefit of this design was that its dimensions made it suitable for use between the poles of the standard 50lb electromagnet, much lighter than the one Randall and Boot employed, and therefore more conducive to airborne operation (see illustration overleaf).

⁵⁵ Callick (1990), p62.

⁵⁶ Le Rossignol and Duke had worked together on high-power transmission valves at GEC before Duke was transferred to Birmingham. Burns (1988b), p277.

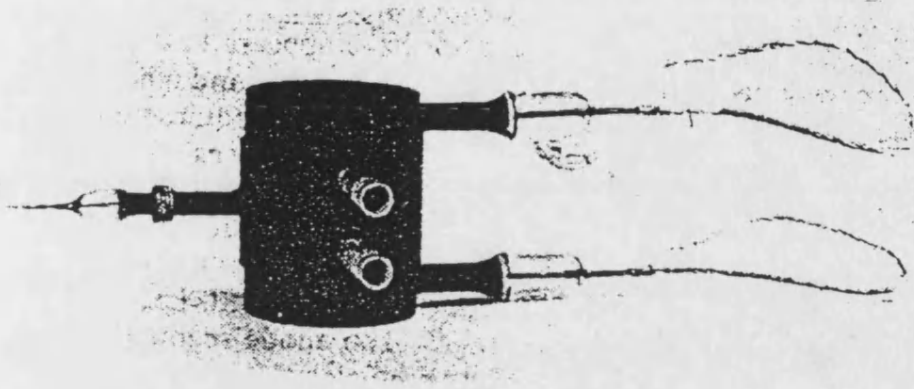


Figure 3.3: Megaw's E1188 pre-production cavity magnetron. From Megaw (1946), p980.

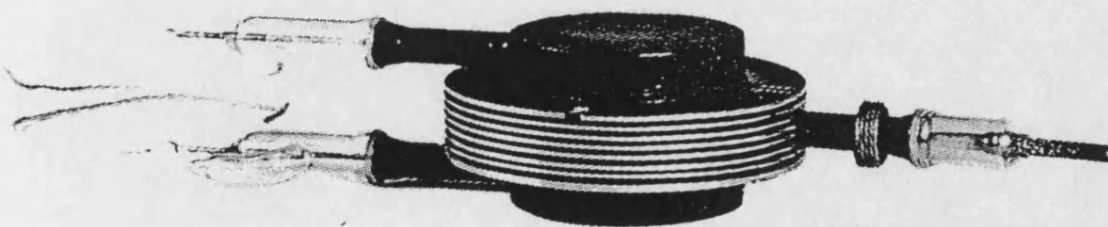


Figure 3.4: Megaw's E1189 pre-production cavity magnetron. Compare to figures 3.2 and 3.3. From Lovell (1991), p40.

As I relate in the next chapter, at this time GEC were working on an Air Ministry contract for a 25cm AI system. They were using “micropup” valves (as used in 1.5m AI, see previous chapter), but had also tasked their valve team with designing a split-anode magnetron with four segments for use in their AI. According to Megaw:

[T]he chief interest in the copper-block structure, so far as the commitments of the GEC Laboratories were concerned, was as a basis for high power c.w. designs for communication on rather shorter wavelengths. But with increasing pressure on the need for 10-cm A.I. it was considered whether a design using this technique could provide a lighter and more powerful pulse source than the dull-emitter resonant segment magnetron which was already in development, with good prospects of producing as much peak power as the Birmingham valve. Both of these, as they stood, involved electromagnets which were inconveniently large for airborne use, one on account of the large gap and the other on account of the high field-strength requirement.⁵⁷

GEC's priorities were different from those of AMRE and Birmingham. They had their Air Ministry contract for a 25cm AI system and were not seeking to use 10cm for this particular application. They only saw the cavity magnetron as being a good prospect for communication usage, and not for radar. As the E1188 stood, it was not suitable for use in an aeroplane. Furthermore, GEC did not see the cavity magnetron as being the key to success in very short wave AI. They believed their own valve would be as good. However, after AMRE and Birmingham brought pressure to bear on GEC, Megaw began to see how he could improve his valve.

Megaw's E1188 design suffered from two flaws that had to be eliminated if it was to be used for airborne developments. Firstly that it was water-cooled, which was impractical for installation in an aeroplane. Secondly it required a very large and, importantly, heavy magnet, though at least this was an improvement on the huge electromagnet used in the Birmingham laboratory. This, again, was far from ideal for aeroplane usage where equipment weight was a primary consideration. Thus Megaw set to work designing another valve:

⁵⁷ Megaw (1946), p980.

It was decided to attempt such a design, though it had to be based on several unproved assumptions. These were:-

(1) That the type of oscillation in the copper-block magnetron was the same as in the Gutton-Berline multi-segment valves. The main point here was that it had been concluded from [Megaw's] interpretation of the mode of operation of the latter, at the time of the visit to Paris in 1939, that the cathode diameter should have no critical effect on their behaviour and that therefore - contrary to the general belief about other magnetrons - large diameter cathodes could be used.

This was important in that it allowed Megaw to use a much higher cathode current, which gave higher output powers - high power-output was after all the object of the exercise. Megaw also admits that he was basing his design on "unproved assumptions", but that he had concluded that the new valve design was not significantly different from Gutton's valves which he had seen the previous year. This was an important intuitive step and again illustrates how previous experience is a major factor in rapid scientific advancement. He continued, listing the other unproved assumptions:

(2) That efficient operation of valves of this type was possible with space-charge-limited anode current, so that increased cathode emission would make possible increased pulse output. At this time (April 1940) there was still no decisive information on this point.

(3) That the mode of oscillation of the copper-block resonant system was such that the wavelength was substantially independent of the axial length.

The final assumption was important if he was to make a reduction in the depth of the valve. Making the valve "slimmer" would reduce the gap needed between the poles of the magnet supplying its field, and hence allow the magnet to be physically smaller, and therefore lighter (important in airborne equipment), in producing the same field-strength. The strength of the magnetic field along the cathode determined the efficiency, and the power output of the valve. This was understood by Megaw from his previous experience of split-anode magnetrons. He also intuitively used a wider cathode (assumption 2), which he believed would also increase output power and on which there was "no decisive information at [that] point". Again, Megaw was intuitively introducing a modification

which he believed would improve the valve, without undertaking any experimental work to back his beliefs. He was soon able to check whether his intuitive beliefs were correct.

Just after Megaw's first design was completed he received an example of the improved French magnetron with the oxide cathode. This modification was the major factor in the improved power output of the French valve. The valve, known as the M. 16, was extensively tested by Megaw to evaluate the beneficial properties of its large cathode. His major discovery about the new design was that once the oscillations began, the anode current could be reduced, and "secondary emission" (of electrons, which came free without cathode heating), led to a much greater efficiency in the valve's performance. Megaw then designed a second cavity magnetron, the E1189 (see previous page for illustration). This was much slimmer than the E1188, and incorporated fins for air cooling. Two samples were constructed. One had a spiral tungsten cathode (No 1), and the other a large oxide cathode (No 2). After construction, both valves were tested to determine their performances, and found to give 1kW of power. Within two weeks (mid-July), he had raised this to 10kW at 9.8cm which was a huge increase in available power from the previous design.⁵⁸

No 1 was sent to the GEC AI group, and in the third week of July 1940 No 2 was sent to AMRE at Worth Matravers. Further samples were constructed, No 4 going to Birmingham on 24th July, and in August an eight-cavity design was completed. Sample No 12 with eight cavities went to the US with the Tizard mission (see next chapter). This was the source of some confusion, through a mistake. The drawings that Bowen had with him showed the original six cavities, but when the valve was x-rayed by the Americans it showed eight. It took a good deal of persuasion, and contact back to GEC to convince the Americans they weren't being misled by British "generosity"!⁵⁹

3.4 Conclusion

The cavity magnetron proved to be a success, as I relate in the next two chapters. This was due to a very fortuitous combination of ideas and experience, made possible by

⁵⁸ Megaw (1946), p982.

⁵⁹ Callick (1990), p64.

a lucky series of contacts. I believe that it was only due to these personal contacts between individuals with specific skills that the engineered cavity magnetron was produced. Much has been made of the achievement of Randall and Boot, and it is certainly true that their idea for combining resonant cavities with the geometry of the existing spit-anode magnetron valve was a major breakthrough in the search for a source of high-power centimetre waves. However, as it stood the Randall/Boot cavity-magnetron was neither more nor less capable of delivering the desired amount of power than the conventional glass magnetrons being developed at GEC and SFR. What really made the magnetron into a suitable transmitting valve for radar was the work done by Megaw. He was the key, combining the insight that Randall and Boot put into anode design, with the work done by the French in cathode design, together with much accumulated experience in engineering valves for many different purposes (See overleaf for diagram).

The cavity magnetron came to be used in the centimetre version of AI that was developed by Air Ministry personnel during the summer of 1940. I have shown that its development came about through the ideas and skills of small groups of people who were connected with each other. These same conditions were pertinent to the development of metric AI described in chapter 2, and as I will show in the next chapter, to the development of centimetre AI.

It was Hansen and the Varians who developed the resonant principle with their klystron. Oliphant brought experience with this valve and the ideas associated with it to Birmingham, where they were transmitted to Randall and Boot. Randall had previously worked with Megaw at GEC. In the interim Megaw became familiar with split-anode magnetron design, and obtained experience with oxide cathodes from Gutton at SFR. he was also familiar with new sealing and manufacturing techniques being developed at GEC. Megaw made a visit to see the Birmingham cavity magnetron, and was able to combine this with his experience of split-anode magnetron design, and Gutton's oxide-coated cathode, to design the E1189 pre-production cavity magnetron that was so useful to the 10cm radar team (see next chapter).

The major factor that made E1189 so useful was its stability, in two senses. Firstly, it had embedded in it the embodied practical skill of several people: Randall and Boot, Megaw, and Gutton. Megaw's contribution was to produce an item that could be

Type and Direction of Knowledge Transfer

- > Personal Contact
- - - - -> Apparatus / Equipment
-> Information (written)

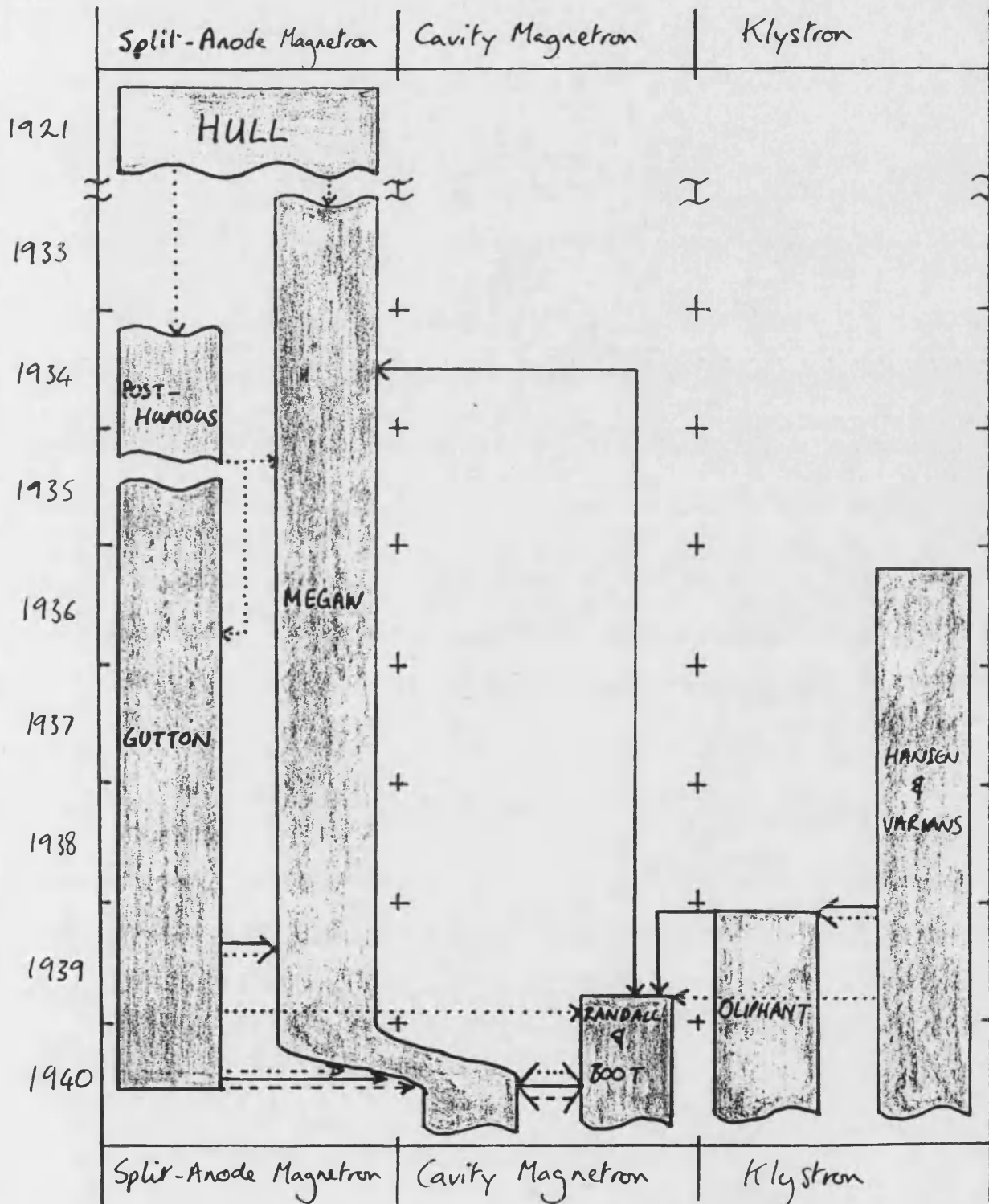


Figure 3.5: The background to magnetron development. I show here how people, places and artefacts were linked in time and space in the magnetron development story. Time is on the Y-axis.

manufactured easily. The performance that the cavity magnetron was able to give could then be easily replicated, both in terms of the consecutive performances of one particular valve, and that it was possible to copy (or replicate) the valve so that each one could be made to perform as it should by persons who did not have the designers' embodied skill. This was the second form of stability, which placed it in marked contrast to the unstable klystron which didn't perform the same way each time.

Chapter 4: British Centimetre AI Radar

4.1 Introduction

In the previous chapter I told how interest in centimetre waves arose, and I described how its various components were developed. In this chapter I will do two things. I will document the story of how the experiments with centimetre radar progressed using those components. I will also draw conclusions about philosophical aspects of that work based upon the analytical framework I developed in the first chapter.

Interest in using centimetre waves for radar had a chequered history until some months into the Second World War. Late in 1939, despite official indifference towards earlier enthusiasm on the part of people like E.G.Bowen at Bawdsey, came a pressure to commence centimetre-wave radar experiments. Air Ministry interest culminated in a contract with GEC for a system on 25cm (although this was based on using conventional components), and Bowen acted as a co-ordinator between AMRE and GEC.

Despite research instigated outside of AMRE by CVD, work on centimetres only began at there in March 1940. It was at this time that Bowen and his 1.5m AI came under pressure to due to the problem of minimum range (see chapter 2). Centimetre research was started by Herbert Skinner, slightly before the AMRE moved to the Dorset Coast at Worth Matravers in May. During the spring and early summer an intense period of activity produced results in both AMRE and GEC. AMRE chose to use the newly developed cavity magnetron as a source for high-power 10cm waves, and GEC continued their work on their 25cm system (despite GEC's laboratories refining the cavity magnetron - see previous chapter). Late summer saw differences between the two groups come into the open, and culminated in a short period where both competed for the upper hand as to who would be allowed to continue the research as senior partner. This dispute centred around whether to go with AMRE's innovative, but still relatively unproved, 10cm work, or GEC's more conventionally based and engineered 25cm system.

The debate was settled in AMRE's favour, and further research was continued there during late 1940 and 1941. GEC now acted in a supporting and secondary role. In March 1941 AMRE researchers commenced airborne experiments with the new set.

They learned a great deal, solving most of their problems by the early summer of that year. The final airborne problem, that of common transmit-receive, wasn't solved until the summer of 1941 when the "soft-Sutton" klystron was introduced. This valve stopped the powerful transmitter pulse entering the receiver, and burning out the delicate components it contained.

At the same time, centimetre components were becoming better understood and engineered. Work on airborne radar applications bifurcated in late December 1941 when the H₂S ground-mapping radar project was started (see chapter 5). This work relied on the understanding and expertise built up with 10cm AI experiments during the previous 18 months. AI finally entered service in a production engineered form in late 1942, nearly three years after the invention of the cavity magnetron.

This radar development work was undertaken by small teams of physicists. I will show that there were factors that aided their success and some that abetted it. Firstly, how were the teams organised? What was their location, the nature of their internal and external contacts? Their ambience? Both the GEC and AMRE teams were small, close knit and composed of people who had spent considerable periods either working together, or in the same laboratory with the same colleagues. They shared common work practices, and were able to relate easily in tackling their problems. I will examine how these conditions influenced the transfer of embodied and embedded knowledge within and between the laboratories interested in centimetre radar.

Secondly, these common work practices and a large measure of acquired practical skill, together with an unconventional approach made possible by using physicists and other scientists rather than radio engineers (who had been "indoctrinated" into ideas of what could and couldn't work by their training), led to rapid success. I will highlight examples of this phenomena, identifying which practices fall into the category of complete embodied "know-how" (as in unarticulable) and which could be articulated when transfer of these skills was required. I will also assess how these fit in with Gooding's claims¹ about the fine-structure of experiment, where the practice of the experimenter leads to the acquisition of embodied knowledge about how to produce phenomena.

¹ See chapter 1, and Gooding (1990).

Thirdly, periods of internecine or external rivalry (and associated non co-operation), and periods of dispersion of group members, led to slow progress. I will assess this with reference to the ideas I introduced in chapter 1 about the communication of knowledge. Non-co-operation is a social inhibitor of the transfer of knowledge, that produce a debate about the veracity of results or effects in the same way that the failure to replicate results can. Whenever there is disagreement about results or an effect, the apparatus and techniques of the original experimenter are questioned.² In conclusion, I will show that the nature of the teams played a very important part in what they achieved.

I will be drawing out aspects of the story that show what practical skills the participants had, and how they learned how to improve their working relationship with their apparatus by actually doing experiments. This follows the what Gooding has written about how both Faraday and Morpurgo and his team refined their apparatus and techniques through repeated experimental trial.³ As we would expect, there are examples of the non-transportability of technical know-how between groups, and also an instance of failure to replicate within a group.

Even during high-pressure war-time conditions it still took a considerable amount of time to produce a finished, stable artefact: a production AI radar. It is important to note that many aspects of the research were not straightforward; there were numerous instances of things not going according to plan, of unexpected difficulties and moments of rapid progress when the experimenter's ability to get to grips with the world through their apparatus led them to make advances without necessarily having a full theoretical understanding of what they were doing. This, in particular, supports ideas put forward by Gooding in *Experiment and the Making of Meaning* that interaction with the world through experiment has a reciprocal relationship with individual and group understanding of the world. In this early stage of centimetre radar development, I wish to show that the production of "good" results, ie building centimetre AI radar that did what it was supposed to do in aircraft, relied on experimental skill. This skill was in three forms: embodied unarticulable knowledge, embodied articulable knowledge and embedded knowledge (see chapter 1). I will identify what stage the apparatus was at with reference to these three forms of knowledge before it was adapted for use as a ground-mapping

² See Collins (1985), ch 4.

³ See Gooding (1992).

Official ambivalence to researching centimetre waves meant that when war broke out in September, the focus for centimetre research was located away from AMRE (as it was renamed), at Birmingham University and at GEC. Furthermore, at the outbreak of war the whole Establishment was evacuated from Bawdsey which was thought far too prone to aerial attack (being very obvious from the air because of its 350 feet high CH towers). As described in chapter 2, the Bawdsey establishment was packed and moved in two days. Watson-Watt had made arrangements for AMRE to move to Dundee University, with Bowen's Airborne Group going to Scone airport some 20 miles away. Whilst the chaos caused by the move for the main radar group at Dundee was bad enough, the Airborne group had things far worse. They had minimal access to facilities and apparatus. For example, obtaining scientific literature was next to impossible and communication with staff at the main Establishment was greatly impeded, as Bowen described:

When he arrived at Dundee, Rowe was greeted by an incredulous Vice-Chancellor who seemed to have forgotten Watson-Watt's visit; he has simply not been informed of any subsequent plans. Being an accommodating soul, he offered the new arrivals two rooms, each 20 feet square, which was all the space he could spare for the several people who were either en route to Dundee or already there. I do not want to elaborate on how the problem was finally solved and will leave those responsible to explain what transpired as best they can. There had been a monumental blunder, from which the AMRE, as it was now called, took a year or more to recover.¹¹

This state of affairs was the product of the hasty evacuation and general panic of the first few days of hostilities, when people expected to be bombed straight away. When the situation settled down there was a further move at the beginning of November, when the Airborne Group was again uprooted and went to St Athan in South Wales (as I explain shortly). However, the rest of the Establishment remained in Dundee, until the two groups were reunited in Worth Matravers in May 1940.

One of the main consequences of the split between the Airborne Group and the Main Establishment was that research into centimetre AI, which was primarily Bowen's idea, ground virtually to a halt. The group became little more than advisory staff to the RAF

¹¹ Bowen (1987), p87.

“double-doorknob” valves.⁷ It did not prove particularly successful, as the range obtainable on non-moving, large, ground objects was only three-quarters of a mile. As Bowen’s team were already getting much greater success with their 1.5m system they abandoned further experimentation on the shorter wavelength.

This is to be expected as the general ambience at the Bawdsey research establishment at this time was not in favour of centimetre wavelengths. Bowen reports that “anyone talking about centimetres was thought of as some sort of crank.”⁸ There were still problems with the 13m Chain Home radars, and the 1.5m AI was also barely off the laboratory bench. Therefore it is hardly surprising that centimetre wave research was viewed as a pipe-dream. Nevertheless, the seeds were sown in Bowen’s mind that very short wavelengths could prove useful.

The first part of 1939 saw the advent of the CVD (see previous chapter), and with it came a greater spirit of co-operation between the Services and their respective Research Establishments. Watson-Watt went to the second CVD meeting on February 14th, and Sir Charles Wright (DCD Admiralty) visited Bawdsey. The Admiralty, according to Bowen, had “expressed [the] greatest enthusiasm for changing to centimetre waves”⁹ (see above), and Bowen and Wright were able to discuss their respective problems during the Spring and Summer. When Bowen explained the problem of minimum range caused in airborne radar by the usage of 1.5m wavelength, Wright agreed that the greatest need for centimetre wavelength transmitter valves was by Bawdsey’s AI team. Bowen calculated that given a beam width of 10 degrees (thought necessary for sufficient resolution to view a target aircraft and eliminate ground returns), and an aperture (aerial width) of about 1m (the diameter of the nose of a fighter aircraft) then a wavelength of approximately 10 centimetres would be ideal (see overleaf for diagram). Wright believed that a similar wavelength would be most suited to gun-laying radar for the Navy.¹⁰

⁷ American companies had pressed ahead with designing miniature valves in the 1930s. The smaller physical dimension of the valves related directly to the wavelength that the valves produced, but making smaller valves required learning new manufacturing techniques, and concomitant investment in training and plant. As British companies were still feeling the effects of the Depression, this investment was not forthcoming. The lack of British expertise in manufacturing these valves led to a severe shortage when war broke out. See Barnett (1986).

⁸ Bowen (1987), p143.

⁹ Bowen (1987), p143.

¹⁰ Bowen (1987), p143.

team hastily installing 1.5m AI into Blenheim aircraft to act as crude night-fighters.¹² This situation was intolerable, and was probably due to animosity between Bowen, and Rowe and Lewis (Rowe's deputy) which arose from the "minimum-range problem" described in chapter 2. Lewis' appointment in the Summer of 1939 "risked offending [Rowe's] old colleagues", namely Bowen and Wilkins, who was Bowen's equivalent on ground radar. Bowen had no great respect for Rowe, whom he thought of as being officious and ignorant of electronics matters. In correspondence with Lovell he wrote:

[F]rom his first appearance at Bawdsey, Rowe was the man who was out of his element and overwhelmed with problems he could not cope with. This was largely due to his complete ignorance of electronic matters and how to conduct electronic research... [T]he appointment of Lewis did nothing to improve the situation. I found Lewis arrogant, and totally unable to discuss a problem rationally if he found himself out of his depth, which was often the case in those early days... In my experience, he always behaved as if he was all-knowing; this may have fooled some of the newcomers but it completely alienated the pioneers.¹³

This period marked the beginning of Bowen's isolation from the centre of AMRE. Apart from the internal politics, it was also irritating to those researchers on the team to be treated in such a way. Such internal strife, as well as causing a level of animosity that strong enough to be remarked upon by persons not directly involved, had a retarding effect on developmental work. Bowen possessed a great deal of embodied knowledge about radar, especially airborne radar. If he was in the laboratory, other researchers could go directly to him for advice about how to proceed, or get him to show them how to do something, or ask him to help design apparatus. His physical absence meant that they lost an important resource.

Bernard Lovell, a young researcher at Manchester University working for Patrick Blackett (Tizard Committee member and former Cavendish Laboratory researcher) was one of several scientists who visited Chain Home stations during the summer of 1939. Lovell was one of those earmarked for radar research should war break out, and he was duly called up. He was amongst those who went to Dundee with the evacuated

¹² It was whilst using these aircraft, designed as bombers, as night-fighters during the Summer of 1940 that many of the lessons of how to actually use AI to make night-time interceptions were learned. See chapter 2.

¹³ Bowen to Lovell, 15/5/37, in W.B.Lewis Papers.

Establishment, arriving on September 29th. His initial instructions were that he should commence work on short wavelength experiments. Together with another Manchester colleague, Peter Ingleby, Lovell set to work on building a transmitter that worked on shorter wavelengths than the 15m apparatus used. However, within a few days they were ordered to assist with the fitting of 1.5m AI into Blenheims. There then began a period of oscillation between occasional scraps of research, and working as glorified fitters. The situation was not conducive to progress as Lovell described:

Working in the small group with this type of conflict seven days a week (all leave and rest days had been cancelled) soon eroded the enthusiasm and hope.

Furthermore, Lovell began to realise that military work was not as straightforward as his University research had been, as he went on to recount:

I had been allowed to join in a test flight of the AI and that quickly made me realise that making an equipment work in the air was quite a different matter from operating on the ground. Hitherto in my few years of University research I had donned a lab coat and dealt with an apparatus in the warmth and quiet of a laboratory. Now the lab coat was replaced by a bulky flying suit, a parachute harness and a parachute that also served as a seat. The spacious laboratory was replaced by the cramped and cold interior of a Blenheim night fighter. The noise made normal conversation impossible and the vibration was so great I could not imagine how any electronic equipment could survive even on its anti-vibration mountings.¹⁴

This state of affairs continued for a while, and at the beginning of November the group was moved to St Athan in South Wales (whilst the main establishment remained in Dundee). Facilities here were, if anything, worse than in Dundee, and there was the added disadvantage of being 400 miles away from their colleagues as opposed to just 20.

Increasingly frustrated by his utilisation as little more than a maintenance fitter, Lovell again contacted his old boss, Blackett:

After only a few weeks at St Athan I wrote to Blackett (17 December) explaining what was happening - but without this hindsight [with respect to the Bowen/Rowe

¹⁴ Lovell (1991), p17.

friction] as to the probable cause - arguing that the work we were doing could be carried out far more efficiently and appropriately by the engineers from the firms who were then manufacturing the airborne AI equipment.¹⁵

Blackett had already visited St Athan on 14th December with Tizard and Watson-Watt, and they were concerned with the situation. Blackett returned on 17th January, but in the meantime Ingleby and Beattie were killed when the aircraft in which they were flying crashed near Bridgend on 7th January. This set back their research, and brought a harsh note of reality to the group. On 26th February Lovell noted that he was studying a paper from GEC on electromagnetic horns, and that there were discussions as to whether the team should move to GEC where progress was being made on 50cm.¹⁶

Blackett's January visit brought a ray of hope into the atmosphere of gloom, as he had news that part of Bowen's group was to form a re-created centimetre research team. This 'part' was to be Lovell, who, following the deaths of Ingleby and Beattie had A.H.Chapman (Blackett's technician) as his sole team member at that time. On 26th February Lovell was joined by Alan Hodgkin, a physiologist by training from Cambridge, whose transfer was arranged by Blackett. On being called up under the plan to bring scientists into military research, Hodgkin's initial posting was to Farnborough, where he worked on the effects of altitude on pilots. Assigning a physiologist to work on radar may at first seem very odd, except that Hodgkin's pre-war research was primarily on conduction in nerve fibres, which had led him to become familiar with constructing and operating electronic equipment such as oscilloscopes. It was whilst he was at Farnborough that he met Blackett, and his posting was arranged.¹⁷

When Hodgkin arrived at St Athan, six months had elapsed between the outbreak of War and the recommencement at AMRE of any kind of investigation into wavelengths shorter than the 1.5m used in AI, ASV and CHL (although GEC were investigating this area independently to AMRE). This period of delay was not caused by experimental problems. It was caused by factors *outside* the laboratory, and indicates how such factors are an important part of the progression of centimetre radar research. I believe that these conditions of confusion were symptomatic of the change in certainties

¹⁵ Lovell (1991), p21.

¹⁶ van der Hulst/Lovell Correspondence, paper by Lovell.

¹⁷ Hodgkin (1992), p141.

occasioned by the Wartime environment. This confusion is a factor which impedes scientific progress, as it leads to the closure of channels of information such as the unavailability of persons or books and journals. It is one of the factors which differentiates how scientific research operates in wartime, and how it operates in peace. I will look at some of what I believe to be the benefits of the wartime environment, such as greater speed in allocation of resources, later in this chapter and also in chapter 5.

4.2.1 Commencement of Centimetre Research at St Athan

When Hodgkin arrived, he, Lovell and Chapman recommenced research into using shorter wavelengths. It was by no means clear that their task was in any way useful, as Hodgkin recalled:

At an early stage Eddie Bowen, who I found an inspiring person, told me that I should give Lovell any help that he needed and explained broadly what he wanted me to do. In a letter to my mother dated 28 February 1940 I wrote, 'I don't yet know exactly what my final job will be. At present I am helping Lovell with a problem which looks as if it's going to be insoluble so perhaps helping is not quite the right word.'¹⁸

They had to start somewhere, so they began by investigating the possibilities of different sorts of antennas:

At last we were allowed to begin a little research. The two of us with Chapman's help made a large horn-type antenna and on 5 March we took it outside the hangar and fed it with a 50 centimetre oscillator.¹⁹

This is one of the rare instances where Lovell acknowledges the role of his assistant, Chapman. Technicians play an important part in the life of a laboratory; they have skills that complement and augment those of the scientists, but their contribution is usually omitted from scientific accounts. However, their different perspective provides the

¹⁸ Hodgkin (1992), p142.

¹⁹ Lovell (1991), p25.

historian with another useful impression of what goes on in the laboratory. This is discussed further in a paper I have written (see Appendix A).²⁰ Despite the usual practice, Chapman's help is also mentioned by Hodgkin:

[Bowen] suggested that I should read up about ways of generating narrow beams - horns, paraboloids and so on, and, if possible, of swinging the resulting beam electrically... I decided to use 50 cm, which was then a reasonably understood wavelength. Lovell's assistant Chapman, from Blackett's Laboratory, helped me to build an oscillator at this wavelength which we used for a month or two.²¹

As they were interested in using short wavelengths the two men went to visit GEC, where they were shown the company's 'micropup' valve, which was capable of generating several kilowatts at 50cm. The micropup was the basis of GEC's 25cm system (see next section), and aroused considerable interest in Lovell and Hodgkin:

Alan Hodgkin and I were fascinated by this new development and for the first time the practical possibility of an AI on a wavelength of 50 centimetres with a narrow beam to avoid the ground returns began to emerge. We returned to St Athan and our very large horn. Even if it were possible to fit such a very large device in a nightfighter some means had to be found, other than moving the horn, of swinging the narrow beam. At least facing a seemingly insuperable research problem with a fellow spirit was a refreshing change from the preceding months of drudgery with the AI and ASV equipment.²²

It is interesting to read what Hodgkin has to say about the same experiments:

Following Bowen's advice, I tested out the beam-swinging method at 50cm using three large wave guides. The physicists at Dundee, whom I was soon to meet at Swanage, felt that my tests were a great waste of time since the result could be calculated with certainty on the back of an envelope. However, the tests made us organise turntables and other equipment for measuring polar diagrams and it taught me something about electromagnetic radiation. It should also be said that the reason

²⁰ In general, a scientist makes things explicit about the process of science that a technician, who has not had the same background and training, would not and vice-versa. See for example Shapin (1989).

²¹ Hodgkin (1992), p150.

²² Lovell (1991), p25.

Bowen wanted everything tested was that the 90° side lobes from a reflector or array giving a narrow beam are nearly always larger than those calculated from simple wave theory. This is important in Air Interception because the echo from an aeroplane at the same range as your own altitude is liable to be swamped by an enormous ground return.²³

Hodgkin and Lovell were trying to measure the shape of the beam coming from their horn. They did this by producing a “map” of the signal, by measuring its strength in various orientations relative to the aperture of the horn. This information was plotted onto a “polar-diagram”, which showed how the signal strength varied with respect to the angle from the aperture (see overleaf for diagram). The diagram was a two-dimensional slice through the output pattern of the antenna. This does not necessarily mean that the diagram was the same in all orientations. What is important was that any signal coming from the side lobe, unless it was weaker than a signal from the main lobe, would not only obscure the main lobe signal, but also, crucially, be indistinguishable from it. In the case of a ground return, this would almost certainly be stronger than that returned from an aircraft, which is why it was important that such side lobes be measured so they could be suppressed. That such side lobes did not behave as theory predicted is exactly the sort of tacit knowledge that experienced men like Bowen could offer to the newcomers. This quote also illustrates that discrepancies between theory and what was understood by the practitioners did exist, and were having to be re-addressed regularly.

The polar-diagram was an important graphical aid towards understanding how different types of aerial actually worked. It was a tool that the experimenters could use to help them improve their aerials in order to get the shape of signal that they believed would work the best. Graphical representations are often an important tool in allowing an experimenter to “make sense” of his discovery.²⁴

In March 1940 moves were made by the senior management to reunite the whole of AMRE in one location. This information gradually filtered down to Lovell, who heard rumours that the team would be moved back to Dundee, or to GEC, or to the Aircraft Experimental Establishment at Farnborough. Finally it was revealed that they would go to Dorset. It was also at this time that Rowe, still at the main establishment in Dundee,

²³ Hodgkin (1992), pp151-2.

²⁴ See for example, Gooding’s analysis of Faraday’s magnetic curves. Gooding (1990a), ch 4.

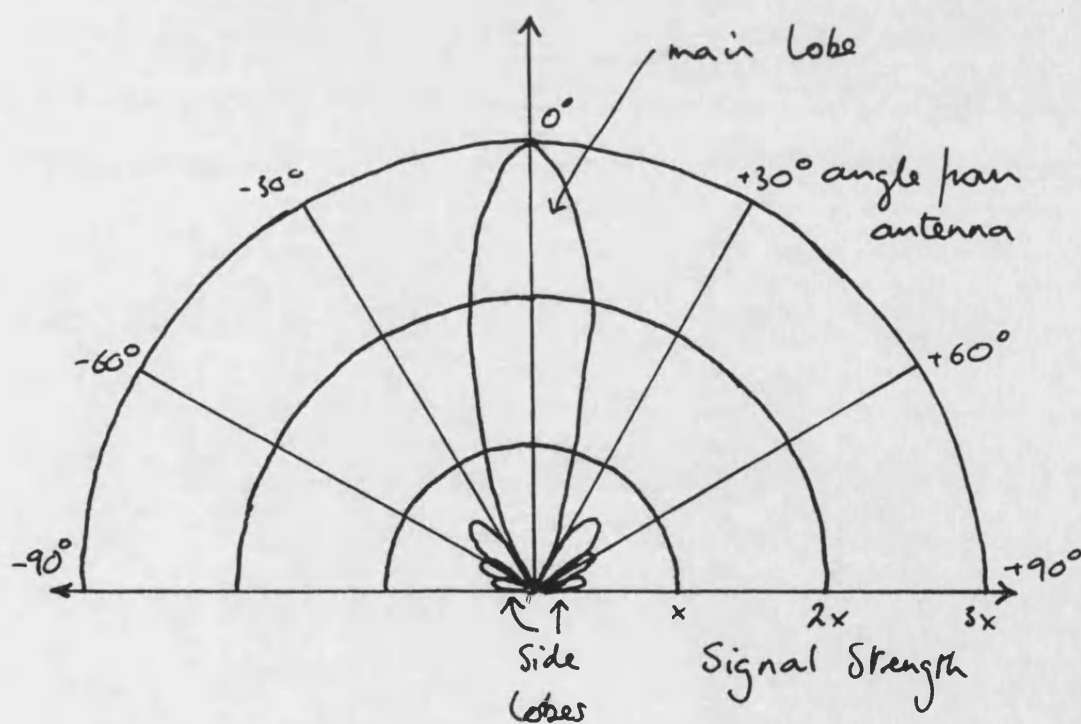


Figure 4.2: A typical aerial polar diagram. The side lobes are the “blobs” that extend out sideways from the main beam. This is a graphical representation of how the signal strength from, or received by, the aerial varies compared to the angle to the aerial in a particular plane.

re-opened investigations into centimetre waves there. He asked Skinner (a Cambridge graduate who had previously lectured Lovell at Bristol) to commence research into this area. Skinner was joined by James Atkinson, who had trained at the Cavendish but had joined Bawdsey on graduation in 1939. When war broke out Atkinson was assigned to modifying the Chain Home stations, and when this was completed in March 1940 he was attached to Skinner. As he recalled:

I immediately reported to the leader of the Centimetre Team - none other than Herbert Skinner, who with his dry and economic sense of humour, informed me that I had doubled the size of the team!

Herbert had been well briefed - I think by W.B.Lewis - on the need for narrow-beam equipment in fighter aircraft to allow them to operate at low altitudes. Although we had no aerial, transmitter or receiver at this stage I was despatched to Stores to order two 6 ft aluminium paraboloids manufactured to ± 1 cm. He was aware of the high-power klystron being developed at Birmingham University, but had no idea when or if we might obtain it.²⁵

It is clear that immediately prior to the move to Worth Matravers the Centimetre Research Team at AMRE was in a very poor state. It consisted of Skinner and Atkinson in Dundee, who had done no practical work at all, and Lovell, Hodgkin and Chapman at St Athan who had done some basic practical work on horn antennas. Those personnel who had experience of airborne radar, namely Bowen and his team, were either sidelined or used as mechanics and fitters to install 1.5m AI and ASV, tasks not connected with centimetre research.

In contrast, GEC were at this time developing their own version of AI. It operated on 25cm and used conventional valve designs. Yet in just a few months, this would be superseded by a 10cm system from AMRE. That this happened was due to the cavity magnetron, whose remarkable development was described in the last chapter. This was despite considerable pressure from within both GEC and AMRE to persevere with something less revolutionary (and, in their eyes, more likely to be successful) than 10cm. In the next section I shall describe the work at GEC.

²⁵ Atkinson (1990), p25.

4.3 GEC's 25cm System

In the previous chapter I explained how it was largely the Admiralty that initiated the work on 10cm transmission valves through the CVD. Bowen was the only person from AMRE with an interest in producing smaller wavelengths to improve his Airborne Interception system. His discussions with Sir Charles Smith of the Admiralty, who was also chairman of CVD, were a contributory factor to the Admiralty's decision to initiate this research. It may seem odd that the push for something perceived to being mostly of benefit to the Air Force came from the Admiralty. However, by this time (October 1939) the Air Ministry too were considering the problem of how to generate centimetre wavelengths at high powers and how they could be used.²⁶

That this interest had sprung up was due largely to the way CVD facilitated the transfer of information about new developments between the different potential interested parties. GEC had a valve, the micropup, which could operate on wavelengths of 50cm at a power of several kilowatts. Through CVD Tizard became aware of the valve's potential, and arranged to visit GEC to see the progress being made there. According to Sir Clifford Paterson's Diary (Paterson was at this time the head of GEC):

Had visit from Sir Henry Tizard who wanted to satisfy himself that all resources of industry were being focused on radio and RDF. He is sure that 'this is the radio war' and radio will win or lose it. He came feeling that some of the Air Ministry problems would go quicker if the centre of responsibility for development were in industrial rather than in Service establishment [sic] as at present. I partially supported him. He inspected our facilities and looked over our valve and CRT work. He is seeing me again after he has seen Watson Watt.²⁷

Through Paterson's membership of the CVD, GEC were in very close contact with developments at the Service establishments, and at Bawdsey in particular. Through his membership of CVD Bowen was also a regular visitor to GEC's Wembley research establishment. Tizard mentioned his own Wembley visit to Watson Watt, who in return mentioned this to Paterson on November 29th:

²⁶ Bowen (1987), p145.

²⁷ Clayton & Algar (Eds. 1991), p11.

Lunched with Watson Watt. He referred to Tizard's visit to us and now wants us to take on research and development for AI - breaking away if possible from orthodoxy - if we can get expectation of better results therefrom. He will visit us here in a few days to discuss and finalise an arrangement. We shall need to incorporate some service people in our group.²⁸

This quote is significant for several reasons. Firstly it supports what I quoted from Lovell's 1991 memoirs, in the previous section. This was in relation to how work at St Athan was getting nowhere, and rumours were circulating of a move to GEC. Secondly, there is another reference to the idea that it would take a "break... from orthodoxy" to solve the problem of centimetre AI. This is a very important theme, as it introduces a tension between the idea of using expertise to solve engineering problems, and the idea that expertise in an area can cloud the ability to make intuitive leaps. Paterson was not the only person with such views. In the previous chapter I quoted Oliphant's view, that physicists would be better able to produce solutions than engineers. Later in this chapter I indicate Batt's initial view that by being a radio engineer he was a misfit in terms of what he viewed as orthodox radio-engineering practice.

Paterson met again with Watson-Watt on December 11th to discuss the AI contract. They arranged a further meeting for 22nd December, which Bowen was also to attend. Before this meeting, on December 15th Paterson saw Appleton who was also a member of the CSSAD. His record of these meetings sheds further light onto why centimetre research was not working at AMRE during this period:

Lunched with Appleton to see how matters stood in regard to his support of our proposed AI work. He is chairman of an RDF sub-committee of Tizard's main committee and is determined to foster the 'farming out' of service problems whenever possible. Furthermore he is concerned at the ineffectiveness of the Service establishments. I said I thought their morale had been undermined since Rowe and the Bawdsey group had been dispersed. They now were no longer an organism.²⁹

²⁸ Clayton & Algar (Eds. 1991), p15.

²⁹ Clayton & Algar (Eds. 1991), p19.

In this quote we see another allusion as to how people viewed the proper way to run a research team at that time - it had to be an “organism”. That is, it had to enable the interaction of its members so that they could produce solutions to the problems facing them much more quickly than when the group was dispersed. Of course, at this time the AI workers at St Athan were not engaging in any research primarily because they were being used to fit existing types of AI instead.

As previously arranged, on December 22nd Paterson met with Watson-Watt, Bowen, Touch and Hanbury Brown to discuss how GEC would approach the problem of developing a short-wave AI under the terms of their Air Ministry contract. At this meeting they decided to bring in GEC’s Television Team to undertake the project. Sir Robert Clayton, who was a member of the Television Team, described himself and them as:

[O]ne of the many groups which reinvented radar when they observed the fluttering of television signals when they were reflected from an aircraft as well as being received directly.³⁰

When assigned to producing an AI on a wavelength of 25cm, the team was allocated new tasks in pursuit of this goal. In the light of Paterson and Appleton’s comments about how teams were better suited towards problem solving, the Television Team was ideal for its new role. The team was led by Dr D.C.Epsley, who, according to Clayton, was a “very good inventor”.³¹ Epsley’s deputy was G.W.Edwards, who was in charge of testing the newly invented systems, and of their flight-trials. Under Edwards were three men: Dr B.J.O’Kane; Robert Clayton, who worked on aerials; and E.C.Cherry, who worked on receivers. They were assisted by D.O.Walter and L.H.B.Knox who drew and constructed units that they designed. There were also other laboratory assistants that they could use. Sir Robert also recalled that the team related very informally, discussing problems together over breaks (in much the same manner as in University research), and were planning to develop their radar in around less than a year (in comparison, by the 1960’s a new system took some ten years from drawing board to production).³² Clayton

³⁰ Clayton (1984), p383.

³¹ Interview between author and Sir Robert Clayton, 10/9/92.

³² Interview between author and Sir Robert Clayton, 10/9/92.

and his colleagues fitted the emerging pattern of the typical radar team very well. Their group was relatively small and close-knit, contained men who were confirmed experimentalists with several years skill in both building and running apparatus, who were amply provided with assistance from experienced technicians. These points are worth bearing in mind when we examine what they achieved, the time in which they achieved it, and this group's similarity to others that came after it in AMRE.

Work based initially around 25cm and the micropup valves that were already being used for 1.5m AI commenced early in the new year. Methods of reception for 25cm were discussed with Bowen and Watson Watt in a general meeting on November 16th. Paterson clearly appreciated the difficulties that Bowen was having at this time, as this comment from his diary shows:

Bowen is a fine chap and I think that our usual collaboration policy will pull things right with the rest of his group.³³

Paterson also mentioned a discussion with Rowe about re-housing the St Athan team at Wembley in his diary:

Had from Rowe [a] private letter re St Athan staff working with Bowen and possibility of our housing some of them here where research is possible under better conditions.³⁴

Given the events described above, the fact that Rowe was discussing the problem could mean one of two things: either he was genuinely concerned over the centimetre team, or he was trying to isolate Bowen even further.

On January 31st 1940 Paterson noted in his diary that:

Epsley tells me that we may have to go down to 5cm for the AI work - anyway the velocity modulation work has to be hastened as in case we may have to both generate and amplify at these wavelengths.³⁵

³³ Clayton & Algar (Eds. 1991), p23.

³⁴ Clayton & Algar (Eds. 1991), p23.

³⁵ Clayton & Algar (Eds. 1991), p23.

Between New Year and the end of January the Television Group studied the available literature on existing microwave components. This study led Epsley to conclude that it would be necessary to go down to 5cm *eventually*. However, according to Clayton, Epsley was still keen to continue working towards the 25cm system even when the 10cm magnetron developments became known. He remained convinced of the viability of the system, perhaps because he felt it more apposite to persevere with relatively better-understood technology than microwaves. By contrast the others at GEC had already become convinced that the cavity magnetron offered greater possibilities for success.

On March 14th Bowen attended a meeting at GEC's Wembley site. At the meeting he pressed GEC to concentrate on 10cm, and not 25cm as per the guideline from the Air Ministry's DCD. He saw the 25cm work as merely a stepping stone to the lower wavelengths that he was convinced were the only solution to the AI problem. He also related, privately, to Paterson that he thought that GEC's efforts with 25cm were along the right lines and that the work would be valuable in any case. By this time Epsley's group had worked hard to produce a "lash-up" which was shown to the visitors from AMRE.³⁶ Careful consideration was given to the scanning arrangements and, not surprisingly given, their background, the team decided on a television-type (up and down, and along) pattern. This is a good example about how scientists will often solve a problem using methods with which they were already familiar. By contrast, when Hodgkin at AMRE designed a scanner arrangement, he chose to use a spiral scanning pattern. Hodgkin was a physiologist. Epsley was also thinking about how they could switch the transmitted and received signals through a single aerial.³⁷ This was a very important problem, that would remain unsolved until well into 1941.

Over the next few months, before news of the cavity magnetron (which was well under way by April 1940) reached the 25cm AI team at Wembley, the team was occupied with investigations into crystal mixers, work into couplings and switches for waveguides, and in further work on improving the operation of their 25cm system. 2kW of pulsed power at this wavelength was extracted from millimicropup valves in April.³⁸

However, the work at GEC changed radically later that summer when AMRE's successful 10cm experiments using the cavity magnetron were made known to them. It

³⁶ Clayton & Algar (Eds. 1991), p31.

³⁷ Clayton (1984), p385.

³⁸ Clayton (1984), p385.

was initially the source of some tension between the two groups. This was later resolved and a good relationship built up, but there was the potential for a block on information exchange between two of the important groups of centimetre radar scientists. This is what Clayton had to say about the subject writing in 1984:

The Government supervision of the [25cm] project was initially in the hands of E.G.Bowen... [who] had developed the airborne interception radar which worked at a wavelength of about 1.5m and which had been further developed and manufactured by EMI at Hayes; liaison meetings were set up between the Wembley and Hayes teams for exchange of information and expertise... [At the same time a]n airborne radar team was established at TRE under Dee... It would appear that the Wembley team was not made aware of this, nor were the TRE team informed for some time of the activity at Wembley. The result was that parallel activities grew up which were to be a source of friction over a substantial period. In spite of this there were considerable contributions from both sides.³⁹

This paragraph establishes two interesting points. The first is that contact between organisations for the purpose of the transmission of *expertise* was viewed as important by the scientists actually doing the work. They realised that contact with other groups was important for them to learn new ways of doing things that would help their own work, and that they could do this best by face-to-face contact and through laboratory visits. They would also pass on their own expertise. This is an example of co-operation between rival firms who would normally jealously guard any knowledge that would give them a competitive edge, and is a distinct reminder of the very different situation that pervaded in wartime. This level of co-operation between firms was never seen in Germany, for example. The second, is that a lack of co-operation could prove to be detrimental to rapid progress, as it opposed the flow of ideas and expertise that facilitate this. Perhaps the best illustration of a beneficial exchange of information between distinct groups is the case of the cavity magnetron, as related in the previous chapter.

The existence of two teams committed to solving the same problem but in different ways led to animosity and non-co-operation until a hierarchical relationship was established between them. This dispute between GEC and AMRE over who had custody of the AI project was eventually resolved by the status of the group leaders rather than

³⁹ Clayton (1984), p385.

by an appeal to the benefits of each system, as I relate in section 4.5.⁴⁰ When it was resolved, the two groups no longer competed without communicating with each other, but shared their expertise.

GEC's 25cm radar team shared certain characteristics with AMRE's 10cm radar team. They were a small, closely-knit group who had worked together for a few years. They were all very experienced in working with electronics, in the field of television. They were able to put this experience to good use as it related very closely to their new task. Their major difference to the AMRE team was that they were pushing relatively well-understood technology to its limits, rather than working with a completely new field. In the next two sections I will cover the development of AMRE's 10cm radar.

4.4 Worth Matravers: the First 10cm Radar

In this section I will do two things. Firstly, I relate the development of 10cm radar in chronological fashion. This allows me to describe the possibilities that faced the researchers as they went along, together with the directions in which they went. Secondly, I wish to show that the 10cm radar that they constructed was an unstable entity which required certain conditions in order to make it work as required. In particular, I want to examine the processes of acquisition of embodied radar-building and using skill by the researchers, and of embedding that skill within their apparatus. This second process was not complete by the time airborne experiments commenced in March 1941, and was not even fully complete when H₂S experiments began in 1942. Due to the chronological description of events I will indicate instances that support the view that 10cm radar was not fully stable by 1941 as they occur. Following the problems that the researchers had is important to this thesis, because I want to show that building 10cm radar was a difficult exercise that involved a lot of work learning how to build and use it on the part of the experimenters. This period of hard work and difficulty should be borne

⁴⁰ This process confirms Collins' ideas about social status and the resolution of disputes. He argues that it is the status of the antagonists within their community which determines whether they win a dispute about knowledge claims, rather than any appeal to the results given by the apparatus. See Collins (1985), ch 4.

in mind when I go on to examine the German centimetre research programme in chapter 7.

The dire situation in centimetre research described in section 4.2 created the necessary impetus for centralising AMRE at one location. The location chosen was Worth Matravers on the Dorset Coast near Swanage (see overleaf for picture). The site was chosen for two reasons. Firstly, it was much closer to London than Dundee and would enhance communication between the institution, the Military and Civil authorities, and the manufacturers. Secondly, the location was thought to be safe from air attack. In early 1940 this was, of course, true; the English Channel came between two allies, France and Britain. However, after the fall of France in June 1940, Luftwaffe forward air-bases were suddenly very close, and AMRE faced the real possibility of air attack. The Germans believed that the British wouldn't be so stupid as to place a top secret establishment in such a vulnerable position, for the site remained unscathed, despite being marked by a very obvious 350 foot high CH mast.⁴¹ Indeed the Germans only attacked the whole CH system once during the Battle of Britain.⁴² They were very successful and put several stations temporarily out of operation, and effectively "blinding" the British air defence. This had the consequence of forcing the RAF to mount standing patrols of fighters for the day that the system was out of operation.⁴³ However, the stations were quickly repaired and were never attacked again.

Work on readying the site began in late March, and by April several AMRE researchers, including Lovell, were informed that this was to be their final destination. The group that was assembled there didn't all arrive together, but most people had come by the second week in May. Skinner and Atkinson arrived early, as Lovell noted in his diary on May 10th:

Spent the afternoon with Skinner and Atkinson planning a 10cm [split-anode] magnetron drive and a scrounging visit to Bristol.⁴⁴

⁴¹ Pritchard (1989).

⁴² Wood & Dempster (1961).

⁴³ Radar was originally designed to prevent the RAF having to mount standing patrols. With radar, fighters could be sent to where they were needed, allowing the pilots to rest, and then putting them directly in the way of incoming raids. This allowed successful defence with limited resources, as the Battle of Britain showed. See Wood & Dempster (1961).

⁴⁴ van der Hulst/Lovell Correspondence, Lovell (unpubl.) 10/5/40.



Figure 4.3: AMRE Worth Matravers, Dorset, 1940. The picture shows 'A' and 'B' sites, 'B' Site being the main complex. 'C' Site was a couple of huts in a field about a quarter of a mile along the road extending to the right (south). From Batt (1991).

Hodgkin arrived “in early May”, but was aggrieved to hear that the airborne team was to be headed by John Pringle, and that Bowen was to be removed (this was in conjunction with the minimum range problem, see chapter 2.).⁴⁵ Philip Dee arrived at Worth Matravers on May 11th, along with several others from his Cavendish research group, including W.E.Burcham, Sam Curran, and Devons. Devons, Dee and Burcham had lectured Atkinson at the Cavendish two years previously.⁴⁶ They came from an establishment in Exeter, working on an anti-aircraft system that involved firing rockets on wires. This was the system that Lindemann had argued for whilst opposing radar in 1936 (see chapter 2).

On May 14th Reg Batt arrived. He was one of a dozen or so people who were recruited to be technicians in the new establishment. Previously he had worked for the Post Office Engineering Department, and answered a low-key advertisement in the Times requesting people for an undisclosed government task. On arrival he was finally told of the role of AMRE. This was explained as designing devices to locate enemy aircraft and ships using radio waves, something that seemed in the realm of science fiction to the young man.⁴⁷ Batt was described by Atkinson as being “there to show us how properly to wire up anything electrical.”⁴⁸ Batt’s descriptions provide a useful counterpoint to those of the scientists, as they are from a completely different perspective - that of a technician. They often have the effect of rendering strange to the reader what to the scientists seemed at most problematic (see also Lovell and Chapman’s relationship, section 4.2.1). Yet despite his different grade and background Batt was accepted smoothly into the team, as his account of his introduction to Skinner and Atkinson written in 1991, shows:

They greeted me with genuine warmth which somewhat took me aback, used as I was to a hierarchical institution such as the Post Office, where a junior grade would be expected to know his place. It seemed I had now entered the more democratic ambience of university researchers.⁴⁹

⁴⁵ Hodgkin (1992), p155.

⁴⁶ Atkinson (1990), p25.

⁴⁷ Batt (1991), pp15-6.

⁴⁸ Atkinson (1990), p25.

⁴⁹ Batt (1991), p18.

In this quote Batt alludes to the special relationship that operated between technicians and their scientist colleagues, a relationship not always readily recognised by the scientists, but one that is still very important. I discuss it further in Appendix A.

4.4.1 The Klystron Experiments: the “Bodge-Up” from the Bog

The centimetre team were allocated a hut and facilities on C-Site, which was some distance away from the main A-Site. They had a simple wooden hut, very basic with “only one stool and a filing cabinet. There were no facilities”, apart from an Elsan chemical toilet which was cannibalised by Atkinson to use as a waveguide for klystron experiments. As well as his improvisational abilities, one of Atkinson’s other gifts was his understanding of the RAF stores system that enabled him to procure items easily when others were frustrated by bureaucracy.

Hodgkin described their hut:

It now seems extraordinary that all this high-powered work went on in a small isolated hut, reached by a muddy track, with no gas, and only makeshift water and electricity supplies.⁵⁰

Ward and Robinson were allocated the task of designing circuits.⁵¹ Not everyone was treated so efficiently, and Hodgkin was initially unsure of what he was supposed to be doing, “except that [he] should design a scanning system for a 10 centimetre Air Interception set.” As his skills were not initially required, he took to spending a lot of time at Christchurch, where he “learnt a good deal about testing radar equipment in a military aircraft”⁵² There were great difficulties with this, not least the variability of the power supply frequency, and the destructive effect of unpressurized, bumpy aircraft on delicate glass components. These were hazards that Lovell had already noted the previous year. Hodgkin returned to permanent residence in Hut 40, ‘C’ Site, at the end of June.

⁵⁰ Hodgkin (1992), p161.

⁵¹ Dee (unpubl.) 15/5/40, in Lovell (1945, unpubl.), in John Rylands University Library, Manchester.

⁵² Hodgkin (1992), pp157-8. He learnt about airborne radar from Hanbury-Brown, who was probably the most experienced person in the country in terms of actually flying with radar equipment.

When he arrived in May, Batt was informed of the various activities going on by the deputy superintendent, Lewis. The 10cm work sounded the most interesting, and Batt volunteered for it. He was allocated as an assistant to A.G.Ward, who was to work on the receiver. Ward explained to him the work that was being undertaken by the centimetre team. This was Batt's reaction to it:

What kind of madhouse had I elected to join? Here was I, having recently acquired formal radio-engineering qualifications, encountering alien concepts such as transit time and resonant cavities - a far cry from broadcast communications.⁵³

10 centimetre work was of a completely different nature to that usually done in radio engineering. It is, therefore, understandable that only physicists, who were free from the engineer's beliefs of what was and wasn't possible, were employed to do the research (see also my comments in section 3.3.2). In a 1990 paper, Batt put it thus:

Facetiously it occurred to me that the powers that be had found themselves lumbered with a bunch of academics who knew nothing about radio, and so had decided to let them loose on this project which respectable radio engineers considered impossible.

I was the odd man out. I had come from the Post Office, was the most junior of the group, but the only one with formal radio engineering qualifications. Yet within hours of my arrival at TRE (then AMRE) I could see that with centimetres, the less one knew about radio the better.⁵⁴

He explained, as he got to grips with new concepts such as electron transit-time, the reason why it all seemed so strange:

To some one like myself accustomed to working at Broadcast frequencies, it was like being told that our motor cars were useless because they could not keep up with a bullet in full flight.⁵⁵

⁵³ Batt (1991), p18.

⁵⁴ Batt (1990), p33.

⁵⁵ Batt (1991), p18.

To underline this point, in a 1990 booklet Burcham described the reaction of the rest of the other AMRE members not engaged in centimetre research:

The group was regarded with amused tolerance by the rest of the Establishment and was allocated a remote hut on a near cliff-top field in which to conduct their university-type experiments for which only the very far-sighted could see any predictable useful outcome.⁵⁶

Burcham's comments highlight the reasons that the centimetre researchers were so different from the other AMRE workers. They were not civil servants, and they were not radio engineers. Nor were they working on refining existing apparatus and techniques. They were University scientists entering a new field of research. I believe that the characteristics that set them apart also facilitated the rapidity of their work, as I will show in this section and the next.

Another perspective on the events comes from Philip Dee, who kept a diary during this period. The entries written about the time of his arrival at Worth Matravers give us his view point about his colleagues, the experiments they did, and his battles with officialdom. In particular he wrote things about his colleagues which, although highly subjective, offer further insight into why centimetre research had been so badly ignored prior to May 1940. He described Rowe as "very self-important and conceited". Bowen, on the other hand, was "in great disgrace over [the] scandal involving the minimum range [of 1.5m AI]"⁵⁷. He was also very worried that things had been so bad at St Athan, as this had got the centimetre group off to a very bad start. His concern probably originated from speaking to Lovell whom he described as "effervescent" and "full of rage" at AMRE's chaos. This chaos, and the personalities of Rowe and Lewis combined to worry Dee as he related:

AMRE is apparently run along the lines in which separate groups working in different places work upon transmitter, receiver, display etc. This might be reasonable for established techniques but is obviously quite impracticable for a new field such as centimetres is likely to prove.⁵⁸

⁵⁶Burcham & Shearman (1990), p20.

⁵⁷It is difficult to judge who was right in this particular case. See chapter 2, section 2.4.4.

⁵⁸Dee (unpubl.) 15/5/40.

Dee clearly had firm ideas about how the group should be run, and saw the importance of interaction along the lines of what occurred in his experience, at University Laboratories (and in particular, the Cavendish Laboratory). Dee had been a supervisor there, to Burcham, Curran and Devons. In the summer of 1939 these three were working on nuclear reactions using H-T equipment. Even though they had acquired some experience with electronics, most of this type of work was done by Lewis. According to Burcham, this was the main reason why Lewis was appointed to AMRE. He remembered that Lewis disappeared during 1939, and he was told that Lewis would be required for government work during the 1939 Summer Long Vacation.⁵⁹ Writing in 1990, Atkinson described Burcham as “quiet and restrained, possibly as a result of previous years spent working with Dee at the Cavendish, but he could be relied upon to get Dee going again should there be a lull - or to warn us what to expect next.”⁶⁰

Skinner and Dee set about trying to persuade Rowe and Lewis, who were “uninterested in AI” against their “Pre-war Civil-Service mentality” to “centre all the work in one place”⁶¹. Dee had also to discover how he was going to fit into the hierarchy at AMRE, such as there was at that time. When Dee was at the Cavendish Laboratory he had been superior to Lewis (who was now Deputy Superintendent). He now found himself to be below Lewis in the chain of command. This reversal in their relationship had the effect of subverting the formal structures of power at AMRE as they applied to the centimetre group. Consequently, they often acted outside the regulations of the establishment, but in the belief that they were circumventing restrictions to rapid progress. This behaviour was tolerated grudgingly if it produced results.

The relationship between Dee, Skinner and the leaders of AMRE was recalled by Burcham, who wrote that Dee and Skinner were officially group leaders under Lewis. Bowen saw Lewis as an active hindrance towards progress, but Burcham believed that Lewis pushed for the centimetre work to be continued during the Battle of Britain and threat of invasion (as did Lovell, and also Atkinson). If this was the case, then he had a positive effect towards ensuring the continuation of the project, at a time when many

⁵⁹ Burcham interview, 15/2/93.

⁶⁰ Atkinson (1990), p25.

⁶¹ Dee (unpubl.), 15/5/40.

other people not directly connected with it thought that the researchers would be better employed on more pressing problems.

Skinner, who came from Bristol University, had a similar level of experience and status to Dee. At the beginning it wasn't clear whether he or Dee was in charge of the centimetre work, although each had their own area of responsibility. Dee was the one who made arrangements, and offered leadership. He was a tidy, meticulous person, who was well organised and "loved an argument".⁶² People went to him for guidance. Skinner was more of a "virtuoso", who dipped into various projects, including using the split-anode glass envelope magnetron to provide a power source for the aerials. He also acquired low-power klystrons from Sutton at Bristol to be used as local oscillators in the receiver, and was "rather quiet and self-contained, and in any discussion you had to decide whether the grunts he emitted were in favour or not of the views being expressed - and in age he was the senior member of the party."⁶³ He built crystal detectors with a considerable degree of skill, carefully fitting the crystal onto the cats-whisker wire, and tapping it carefully with his pipe.⁶⁴ He had the process worked out by July (see overleaf for illustration):

Skinner made v. nice glass sealed crystal, the great advance being his discovery that Si [silica] can be brazed to rod. This enables glass seals to be made without disturbing crystal mounting.⁶⁵

This process was also described by Batt, who, as a technician, appreciated Skinner's practical abilities:

[Building crystal detectors was] a task taken on by Skinner... who had not only the vision of what was required but the practical abilities, including glass-blowing skill, to make [them].⁶⁶

These two quotes illustrate two points. Firstly, Skinner was highly skilled in a variety of practical arts, that enabled him to construct the apparatus and components necessary for

⁶² Atkinson (1990), p25.

⁶³ Atkinson (1990), p25.

⁶⁴ Burcham interview, 15/2/93.

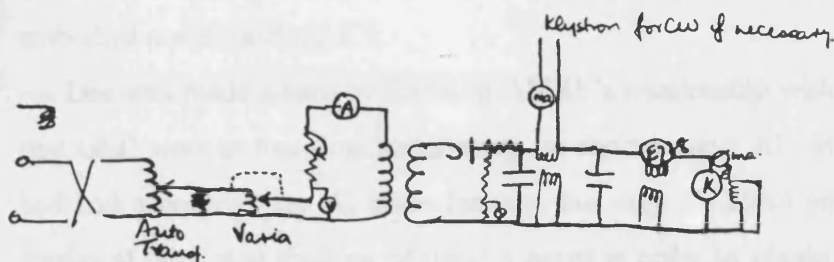
⁶⁵ Burcham (unpubl.), 16/7/40.

⁶⁶ Batt (1991), p47.

Tuesday 16 July 1940

Put up spot welder.

Mounted up low cap. filt. transformer & varac in
 try of H.T. transformer. Note varac must not be
 used on CW. left old filt transformer in circuit
 which is now -



Broke down second Foster filt transformer on surge.
 - ought to have spark gap protection - put in Philips
 transformer.

Ran on pulses - some difficulty in getting it at
 first, but finally got good output under following
 conditions

	1	2
HT.	3 x 1700	3 x 2800
I _g volts	95	175
Ht Volts (new net)	20 scale	max.
ma.	1.5	3
Screen volts	< 500	< 600
Crystal pickup	20 μ A at $\frac{1}{2}$ "	40 μ A at $\frac{1}{2}$ "

Skinner made v. nice glass sealed crystal, the
 great advance being his discovery that ~~the~~ Si
 can be brazed on to rod. This enables glass
 seals to be made without
 disturbing crystal mounting.

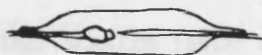


Figure 4.4 Burcham's laboratory notebook, 16/7/40. At the bottom of the page he shows what Skinner's crystal looked like.

his experiments. Secondly, both quotes show that Skinner's practical ability was a part of the discovery process for him. His ideas about how to proceed were shaped by his understanding of how to *make* equipment. Although Skinner was probably the most skilled practical experimenter in the group, his abilities were by no means unique. All the other members of the team had similar experiences and skills, if to a lesser degree, which actually shaped the direction in which they went. They constructed their apparatus and developed their knowledge of how to use it through their physical interaction with it and embodied understanding of it.

Dee was made aware by Rowe of AMRE's relationship with GEC, and of the work that GEC were at that time undertaking on shorter wave AI. He understood that GEC had had a contract for AI since January, but only on 25cm and with a horn antenna. Epsley at GEC was thinking of using 4 horns in order to obtain directional information. Dee noted in his diary that AMRE were more in favour of 8-10cms, but that at this time GEC were pessimistic as to whether there would be ever be enough power for it to work effectively. When Dee wrote this, the animosity between GEC and AMRE was not yet developed. It also illustrates that there was communication between the two groups, which could be of benefit to each.

There was another group interested in centimetres, of course, at Birmingham University. Dee was also informed by Rowe of some of the work going on there:

Oliphant is said to have developed a klystron giving a kW at 8cm and Randall also in Birmingham has a magnetron on the same wavelength. There are no transmitting valves at all in AMRE on such wavelengths and Skinner and I have decided that I should go to Cambridge, and try to get a klystron made in the Cavendish to Oliphant's design as quickly as possible.⁶⁷

Other sources (such as Lovell (1991)) suggest that the magnetron was a complete surprise to the centimetre team when it arrived in July, but perhaps Dee, being more senior, was informed earlier. The quote also illustrates the Cavendish Laboratory links at work.

Another ex-Cavendish member of the group was Burcham, who recalled in interview that the group got together over 15th to 18th May 1940. He shed some light on the

⁶⁷ Dee (unpubl.) 15/5/40.

working practices used at the Cavendish that were also adopted at AMRE due to the high proportion of former laboratory members there. At the Cavendish they worked independently, on separate projects, but regularly interacted with each other to discuss their day to day problems. This way of working continued at Worth Matravers, where the lay-out of their hut mimicked laboratory conditions in allowing easy communication between the researchers. It was a long hut with a bench down each side. There was an office at the end shared by Dee and Skinner, who were not very compatible as Skinner's untidiness infuriated Dee. Further evidence of the way people in close confines can interact was given by Dee's comment on July 5th that Skinner's general untidiness was a problem for Robinson, whose group he and Dee belonged to. Burcham however makes note of Dee and Skinner being incompatible, so maybe this was how Dee vented his irritation with Skinner - by blaming it on someone else. The degree of freedom of communication allowed within the team is surprising in the light of their secret military work, but Burcham believed that it contributed greatly to their rapid success. The whole group were extremely close-knit. Regular interaction was of great importance. They kept little paperwork, apart from technical references, and generally wrote very little down. This is confirmed by Dee when he makes note of writing a report to Rowe, as I show in a later quote.⁶⁸

Over the next few weeks after their arrival in mid-May, Dee set about initiating work on 10cm. On 21st May he went to Cambridge to see Bragg (the head of the Cavendish Laboratory) about getting a klystron built there. On 22nd he went to Birmingham to see their klystron and also, according to him, the cavity magnetron. On 23rd he went back to Cambridge to check on their progress with the klystron. On the same day Lovell "started active attempts to get a 10cm [split-anode] magnetron going in the field."⁶⁹ Meanwhile Dee was confirming how he fitted into the structure, and recorded that on 27th he had a meeting with Rowe to determine where and under whom he should work.

By the end of May conditions appear to have improved in Dee's view, as on 30th he "moved into Hodgkin's aerial hut and Burcham and [he] put together some HT gear for the klystron." The next day "Burcham and [he] had a fine day putting up pumps, getting 3-phase off the mains and running the high vacuum backwards. Quite like old times."⁷⁰

⁶⁸ Burcham interview 15/2/93.

⁶⁹ van der Hulst/Lovell Correspondence, Lovell (unpubl.) 23/5/40.

⁷⁰ Dee (unpubl.) 31/5/40.

As Dee and Burcham had worked together in the Cavendish, they would have established a rapport that allowed close co-operation. This is an example of how they sought to recreate the working environment that they felt suited them, and that they believed increased the speed with which they tackled their problems.

Whilst Dee and Burcham worked towards setting up their klystron, Lovell noted on 29th July that his split-anode magnetron was showing signs of working, and that Dee was building a klystron inside the hut. Lovell's magnetron was "going on 11cms" on 30th, which was a big step towards him being able to commence his aerial experiments.⁷¹

Early in June Dee went to a meeting at GEC, Wembley, to discuss their AI system. He described them as being "attached to 25cm", and as regarding AMRE as simply "a place that supplies aeroplanes for them (GEC) to use for their own gear". On his return he informed Lewis that this was "an inversion of the proper state of affairs". He told Paterson that in his belief, GEC and AMRE should work together and be a joint team. In the same entry he also made mention of the way he was fitting into the establishment, and his views of Lewis and of what he was supposed to be doing:

Lewis... seems to be a little hedgy perhaps for fear that Skinner and I are showing signs of absorbing too much work and perhaps becoming too powerful.⁷²

Lewis, of course, had only the previous year been Dee's junior. This quote shows that even with a pressing external threat, and working in wartime, social relations within the group could have an effect on that group's performance. This is reinforced by Dee's note that he also pressed Lewis to say what he was supposed to be responsible for. He and Skinner, being the senior men in the centimetre group and with no official demarcation between their activities and spheres of influence, were starting to tread on each other's toes. Skinner was deemed by Lewis to be in charge of basic research, and Dee was responsible for putting the system into an aeroplane. However, Dee couldn't see this happening for at least six months, and Pringle had been given that job anyway. As he lamented:

⁷¹ van der Hulst/Lovell Correspondence, Lovell paper.

⁷² Dee (unpubl.) 5/6/40.

[A]s far as I can see all that I am really supposed to do is ask Pringle to get it [installation of centimetre AI] done. This should not take six months.⁷³

Despite all the muddle and confusion in setting up the new establishment, something was being achieved at AMRE during this period. Whilst Dee and Burcham were working on completing the klystron set-up:

Lovell has got a low power 10cm glass envelope magnetron working in a 'field' coffin for the polar diagram measurements... much to Lovell's annoyance everyone twiddles the knobs on every possible occasion, this being the first working apparatus.⁷⁴

This description gives the reader an impression of a cosy, familiar laboratory where everyone is interested in everyone else's work, something that Dee believed to be very important for the new field that they were entering (as illustrated in an earlier quote). The impression of chaotic endeavour is reinforced in a letter by Dee from 1982:

I think that you will never appreciate how unorganized [sic] and chaotic the whole set up was. We went our separate ways but seemed to coalesce when there was a common agreement - or enemy!⁷⁵

Often at this stage the "enemy" was GEC, not the Germans. As GEC's 25cm system took shape and was shown to influential people within the Air Ministry, and the prospect of invasion grew greater, AMRE's 10cm project came under threat of cancellation.

Lovell's own notes confirmed that he was able to make polar diagram measurements of the horns fed by the magnetron on 5th June.⁷⁶ The previous day he had made no measurements because "Atkinson was messing about with pulsing and Skinner with crystals."⁷⁷ Skinner and Ward were working on the receiver, using a klystron from the Clarendon Laboratory at Oxford University as a local oscillator. This became known as the Sutton tube after its inventor, Dr R.W.Sutton.⁷⁸

⁷³ Dee (unpubl.) 5/6/40.

⁷⁴ Dee (unpubl.) 5/6/40.

⁷⁵ van der Hulst/Lovell Correspondence, letter Dee to vdH, quoted in letter vdH to Lovell, 7/2/82.

⁷⁶ van der Hulst/Lovell Correspondence, Lovell paper.

⁷⁷ van der Hulst/Lovell Correspondence, Lovell (unpubl.) 4/6/40.

⁷⁸ Hodgkin (1992), p161.

My account emphasises the level of practical way in which the centimetre researchers learned their skills, and built their apparatus. Because of the need to work quickly the majority of the learning that was done by the researchers was passed on directly through close co-operation within the laboratory hut. However, some of the other aspects of University life used to share information within a group were utilised at AMRE. Dee noted that on June 10th he went to a lecture given by Bowen, the second such lecture in fact, on 1.5m AI and features he believed would be required by centimetre AI. He described Bowen, who he clearly impressed him, as a “terribly competent man”. It is also possible to infer from this quote and his hostility towards Rowe and Lewis, that he was also *unimpressed* that Bowen, a man with clear experience to offer, was being excluded from playing a full role in their work.

In the entry for the 10th June he also mentioned their continued attempts to conjure up something from the klystron. A large part of the delay in their experiments was that they had to wait for a water supply for cooling to be rigged up. Either due to the level of confusion in the new establishment, or because of the slowness of official channels in a Government establishment, nothing was done for several days after their request was made. In the end they went to Cambridge and “stole” the necessary equipment. Despite their attempt to speed up the process by using this unorthodox method of acquiring equipment, rigging it up took seven men ten days. In disgust, Dee wrote that “the whole thing could have been done by Lincoln at the Cavendish in an hour.”⁷⁹ Whilst not stating so explicitly, it would appear that this is a reference to his Laboratory assistant. Such references from scientists are rare (although this was written in his diary, not a source that was intended for publication) and it hints at the intimate relationship, confidence and understanding that the scientists had with their technicians. It also indicates that part of the reason for the friction at AMRE between the University scientists such as Dee, and the civil servants such as Rowe, was their completely different methods of working. These frictions were exacerbated by the unusual circumstances of high pressure to achieve results quickly caused by the War situation. This pressure caused the peacetime structures and methods used at AMRE to be put under severe strain.

Ward and Batt moved into the hut during mid June, joining up with Skinner, Dee, Burcham, Lovell and Chapman, who had moved in slightly earlier. Batt was amazed to

⁷⁹ Dee (unpubl.) 10/6/40.

see the klystron equipment, which was “reminiscent of those early Frankenstein films.”⁸⁰ Batt wrote that the AMRE klystron was built from drawings furnished for Dee by Oliphant, who then had the valve constructed in Cambridge. In some respects one could say that this confirms Collins’ hypothesis⁸¹ that replication cannot be achieved solely by the possession by the replicator of *algorithmic* knowledge (such as diagrams). However, as the klystron was still not a stable entity at its source (Stanford), there is no way to tell whether the subsequent transmissions of knowledge to Oliphant and then to Dee furnished each with the necessary experience to replicate the original work. If the original experimenters had not learned to make the klystron work as they wished every time, then it is to be expected that the replicators would have the same difficulties.

On 12th June Lovell started working on parabolas instead of the horns, and noted the results in his laboratory book:

[M]oved the dipole across the mouth of the sectional parabola and found that it shifted the beam 8° per 5cm... not enough power to see what the side lobes were like... this makes me regard the aerial problems as 75% solved.⁸²

When the valve blew the next day (13th) he phoned up Megaw (at GEC) to see whether another could be found. Thus despite the developing antagonism between GEC and AMRE, there was still evidence of some beneficial co-operation between the two organisations. However, Megaw was not part of the 25cm team at GEC.

June 13th was the day when they finally produced power from their klystron set-up. Up until now the klystron had been incredibly difficult to operate. As Batt put it:

It seemed that the although theory was simple enough, for the klystron to build up its powerful oscillation every facet of its construction, assembly and adjustment had to be just right. Here was the bane of many a scientist - an entity possessing an infinite number of variables.⁸³

⁸⁰ Batt (1991), p40.

⁸¹ Collins (1985), ch 3.

⁸² van der Hulst/Lovell Correspondence, Lovell (unpubl.) 12/6/40.

⁸³ Batt (1991), p41.

Batt's quote provides good evidence to back up Gooding's assertions⁸⁴ that theoretical knowledge is not sufficient to conduct experiments. The experimenter has also to have practical experimental skill: "every facet of its construction, assembly and adjustment had to be just right." In other words, the experimenters had to have spent time working with their equipment, absorbing practical, unarticulated "know-how", in order to get it to work.

Burcham also noted that the klystron first oscillated on June 13th.⁸⁵ One of the main problems with it was that the filament often burnt out and had to be replaced, which meant dismantling the valve as he described in his notebook:

Whenever I am out of the lab and Skinner has to do this he forgets to turn off the water before pulling off the cooling pipes with the result that I am standing all day in about 1/2" depth of water, and the water on the bench is about equally deep but has its surface relieved somewhat by floating cig-ends, tea leaves, banana skins, etc. However, we have managed to get quite a lot of power out of it and light a Pea lamp nearly a foot away.⁸⁶

The klystron remained difficult to use even until the cavity magnetron arrived in July, which was the main reason why the cavity magnetron was so quickly employed. Burcham noted, as late as July 15th, that he had:

Tried pulsing of klystron again. Got it going, but it then suddenly stopped & would not work for sometime. Finally got it again by heating cathode v. hot.⁸⁷

This quote shows the learning process that Burcham was going through with the klystron. He was still not in the position where he fully understood it, but he had by now developed ideas about how to fix it when it went wrong or refused to operate. In other words, he had already learned enough about klystron operation to know what type of remedies might work in getting it going again. But the klystron was clearly far from unproblematic, so we can conclude that at this stage the embodied knowledge of the experimenters in operating it was not embedded into it.

⁸⁴ Gooding (1990), ch 4.

⁸⁵ Burcham (unpubl.), 13/6/40.

⁸⁶ Dee (unpubl.), 13/6/40.

⁸⁷ Burcham (unpubl.), 15/7/40.

Throughout June work continued in the hut, where a routine was developing. Dee gave a very different impression of life in the laboratory in his diary notes, than would normally be found in experimental descriptions published in scientific textbooks or papers. The impression of a lively laboratory is reinforced by this excerpt:

Lovell is now measuring polar diagrams on 10cm in the field outside the hut. His mirrors [parabolic aerals] provide excellent targets for competition between Atkinson and myself of the throwing of large lumps of mud which collect outside the hut.⁸⁸

One can only imagine that this must have irked Lovell considerably!

Lovell was not on his own in performing his experiments. He was being assisted by Chapman, the technician who had been brought from Manchester. Batt pointed out how well Lovell and Chapman complemented each other:

In Chapman, Lovell had a most valuable asset. A university laboratory assistant was truly a jack of all trades, and master of them all. He combined the abilities of an instrument-maker, a tool-maker, a glass-blower, and an expert in vacuum technique. In addition he would have a working knowledge of electronics, optics and thermodynamics. To a university laboratory a good assistant would be as valuable as a master chef to a maître d'hôtel.

Chapman was all of these things but above all he was by temperament calm and unflappable and in no way intimidated by Lovell's hyperactive zeal. Hence they made an effective and extremely productive team.⁸⁹

Batt, as a technician, would be expected to be more aware of the assistance that such people give in the laboratory. In practically every other account given by a scientist, the technician is almost invisible: rarely, if ever alluded to. Here we can see precisely how much of a contribution they could make. This is not to say that scientists did not also have these skills for some of them did, notably Skinner. But there was definitely a partnership and division of labour occurring (see also Appendix A for further discussion of this process).

⁸⁸ Dee (unpubl.), 14/6/40.

⁸⁹ Batt (1991), p43.

On his return to Worth Hodgkin was allocated the task of designing a scanner for the system:

Someone, probably Dee, suggested that I get a prototype scanner going, first on the ground and then in an air-to-air test to take place by Christmas [1940]. This was a tall order, as we then had no way of transmitting and receiving on the same aerial. Nor did we know the answer to much simpler questions such as how to make a satisfactory rotating joint in a concentric cable or wave-guide carrying electromagnetic waves at centimetre wavelength.⁹⁰

In this paragraph Hodgkin uncovered the problems that were facing every member of the team in one way or another: that they were working to a very tight schedule, and very often did not know whether they would be able to meet it. However this pressure forced them to take risks which, surprisingly, often paid off. In mid July Hodgkin met A.W. Whitaker from Nash & Thompson, who gave him a lot of advice on how to build his scanner. This advice was to prove invaluable to Hodgkin, whose successful design relied heavily on the knowledge he gained from Whitaker. This is a good example of the transmission of articulable embodied knowledge by personal communication.

Dee's privileged knowledge of the new cavity magnetron increased, when he learned on 20th June, from a letter from Paterson to Rowe about GEC and AMRE co-operation, that the new magnetron now gave 2 kW. The others were unaware of this, but Dee now viewed their klystron work as a chance to learn about centimetric wavelengths and their behaviour:

With the klystrons we are learning the elements of aerials and transmission lines, much to the amusement of a fat old man called Bartlett... he knows all about the subject but regards it as quite pointless to let us into the secrets. Tried every... way to get some answers from him but gathered that I shall not be ready to understand anything for another 2 or 3 years since the subject is so complex.⁹¹

Despite Dee's sarcasm, one can see the gulf that existed between the majority of the AMRE staff and the strange group at C-Site who persisted in their odd experiments that

⁹⁰ Hodgkin (1992), pp161-2.

⁹¹ Dee (unpubl.), 20/6/40.

were likely to lead nowhere. Support for this view of the existence of a gulf comes from Hodgkin, who commented in 1992 that “[a] curious feature of the build-up was that Dee’s team was largely composed of physicists and we had hardly any mechanical or electrical engineers.”⁹² I noted previously that this meant the researchers did not have any preconceptions about what was or not possible. Certainly when we see what Batt wrote earlier about centimetric research seeming very strange to a radio engineer, this appears to be the case.

On 26th June Rowe finally confirmed Dee’s status as being in charge of installing “AIS” (centimetric, S-Band, AI) into an aeroplane. Dee complained again that this was highly unlikely to occur for a further six months. Further discussions with Rowe on July 12th resulted in Dee being told he was actually in charge of all centimetre research. This elevated Dee to a position superior to Skinner. It went some way to removing the friction in their relationship, as the two had hitherto been treated as having equal seniority by the more junior members of the team. Later on the two went in to different spheres, with Dee in charge of Applications, and Skinner in charge of basic research on 9cm and 3cm.⁹³

The AMRE group’s relative lack of success up until the end of June was in contrast to Birmingham University, who had worked with the klystron since the previous October. they had also, of course, developed the cavity magnetron. Their klystron equipment and transmitter was brought down on July 3rd by Oliphant and Sayers and their group, and demonstrated to the AMRE staff, as Dee noted. Burcham, however, thought it was on July 2nd. He informed me in interview that it was far more refined than the AMRE klystron, but still worked using a pump. As far as I am aware this was the first ever working 10cm system (see overleaf for illustration) - but there is no mention of it anywhere apart from in Burcham & Shearman’s 1990 description. I suspect that this is due to the subsequent success of the cavity-magnetron powered set produced by AMRE.⁹⁴

When compared to the Birmingham team’s success, AMRE’s klystron equipment was still going nowhere. Atkinson wrote that:

⁹² Hodgkin (1992), p170.

⁹³ Hodgkin (1992), p170.

⁹⁴ Burcham interview, 15/2/93.

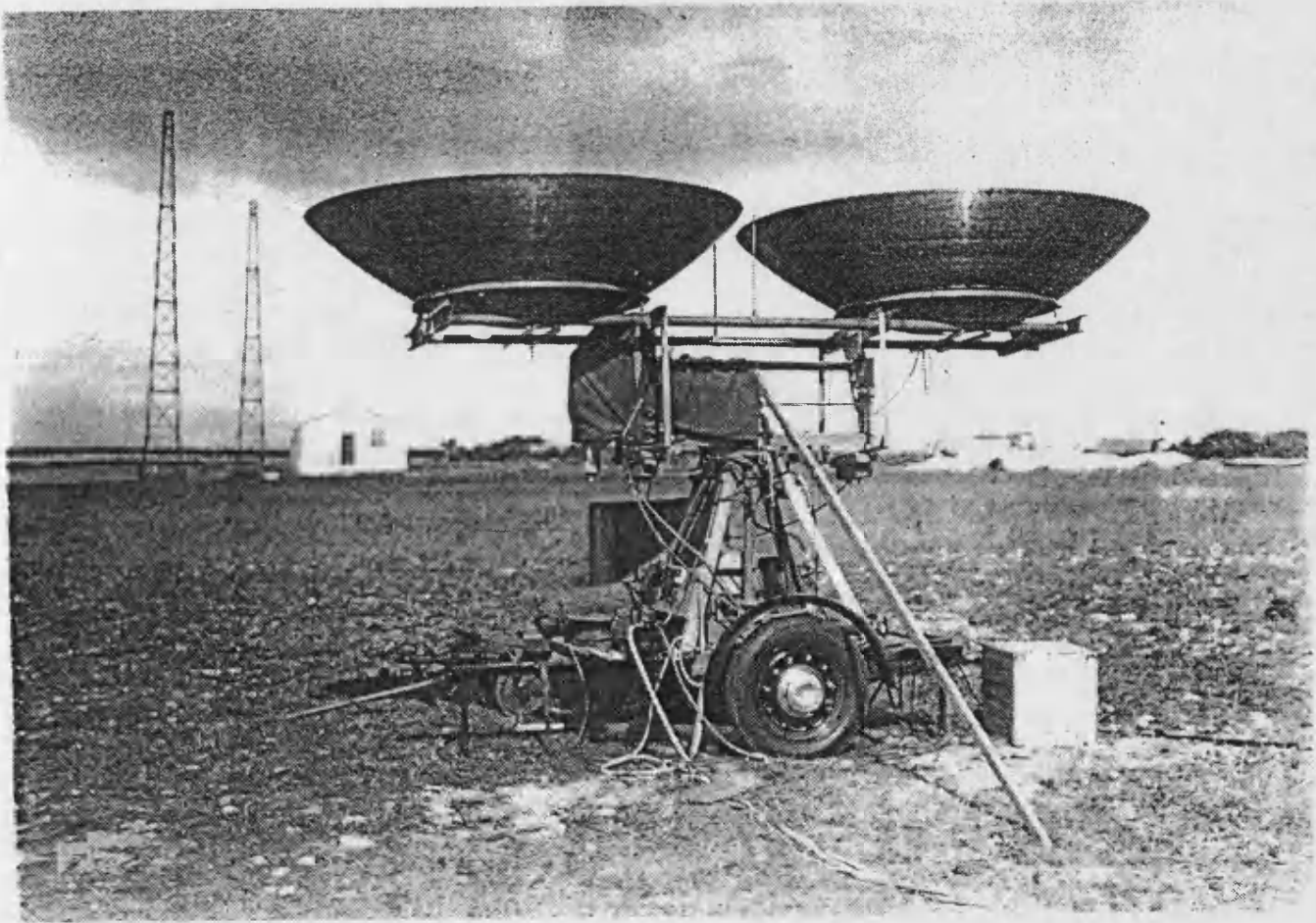


Figure 4.5 Birmingham University's trailer-mounted Klystron set. From Burcham & Shearman (1990), p12.

The project [was] in difficulties. With our relatively massive equipment, any number of variac transformers, Avometers and oscilloscopes, the idea that even if we were able to get echoes at a reasonable range, we would be able to fit it into a fighter aircraft seemed most unlikely, not only to visiting Service personnel, but also amongst many of our own colleagues working on other projects.⁹⁵

This quote indicates that as time progressed, confidence in the AMRE team's ability to produce a centimetre AI was decreasing. In order for them to survive, they had to produce *demonstrable* results that were as good as or in excess of those being produced at GEC and at Birmingham.

Dee was unhappy with the co-ordination of effort on the development of centimetric equipment. On July 9th he had a meeting with EMI, who had been contracted by the Ministry of Aircraft Production (MAP) to work on valves, aerials and receivers:

Presumably Warmsley at MAP is the only co-ordinator between the firms and AMRE and he never touches a piece of apparatus, just tries to direct the work from an office desk in London.⁹⁶

Despite being aimed at bureaucratic interference and incompetence, this is a revealing comment as it further underlines the implicit belief by Dee that the scientists had to get to grips, literally, with their equipment in order to fully appreciate what they were doing and would be able to do. Dee's concern about the situation at Standard Telephones and Cables, which was worse than at EMI, also illustrates further implicit belief in the need for close co-operation between all the various parties concerned with development, when he described them as "obviously need[ing] intimate contact with the other centres of such work."⁹⁷ Again, on 13th July 1940, he was very disparaging about a bureaucrat: "DCD [Sir George Lee] is here for a meeting on AIS [10cm AI]. He is a hopeless old man with no knowledge of any of us." Dee firmly believed that there were very great problems in this area, as a 1981 letter shows:

⁹⁵ Atkinson (1990), p26.

⁹⁶ Dee (unpubl.) 9/7/40.

⁹⁷ Dee (unpubl.) 10/7/40.

The brutal fact was that in early 1940 people at MAP were thinking they could develop a 10cm AI by sending secret, limited information out in different directions - to firms, to RAE, to TRE etc., etc. The apparent idea was that all this information would come together at MAP and an equipment evolve. It was just ludicrous and neither Skinner nor I would take part in such a game. Instead we went ahead to make an equipment, pulling in firms as required.⁹⁸

Dee and Skinner's resistance to this policy of what they saw as excessive secrecy shows that they believed that close co-operation was essential for rapid progress. They realised that it was important for the people actually building and experimenting with the equipment, and those who would manufacture it, to be closely involved at all stages of the process. However, at first they had to pursue this policy against the inclination of the organisations with which they worked. It was a measure of their status that they were allowed to work in this fashion. The policy of openness was eventually enshrined in Rowe's weekly "Sunday Soviet" meetings (see next chapter for an explanation). It is also interesting to note that the process that they describe as being so detrimental, the "sending [out of] secret, limited information... in different directions", was exactly the process used in German radar research, as I relate in chapters 6 and 7.

One of the effects of the centimetre team's policy of autonomy, was that the researchers were expected to take responsibility for the equipment they were building, as Hodgkin related:

In 1940 when I was still a junior scientific officer earning £300 per annum I was encouraged to write and sign letters to firms on technical matters, and Dee's authority was required only if some point of major policy were involved.⁹⁹

As the system was also subject to a weekly review, this stopped over-enthusiastic young researchers from going too far, as Lovell did in the winter of 1940. He ordered several thousand square feet of perspex in order to construct a covered "greenhouse" within which to perform aerial experiments without being exposed to the elements. He reasoned that Air Ministry procedures were such that ordering a large amount of material was as

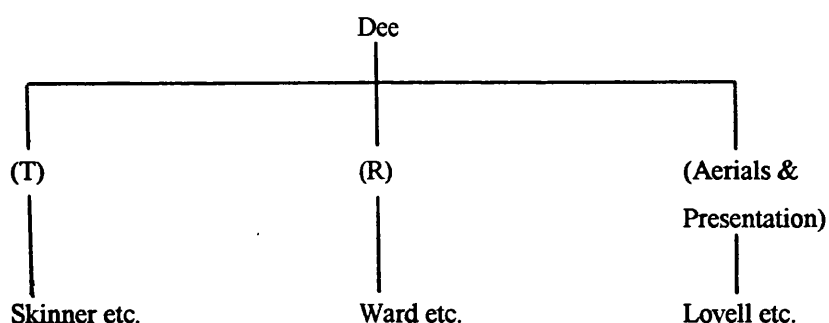
⁹⁸ van der Hulst/Lovell Correspondence, letter Dee to vdH, quoted in a letter vdH to Lovell, 7/2/82.

⁹⁹ Hodgkin (1992), p171.

easy as a small amount. When this misdemeanour was discovered, Lovell was reprimanded for overstepping the mark.

The result of Sir George Lee's visit on 13th July was that the relationships within the 10cm team were unofficially formalised. Burcham recorded this relationship in his notebook:

I worked with Atkinson on pulsing the klystron, with moderate but not especially great success. A family tree has now been drawn up for 10cm work on the following lines (unofficial):



DCD is coming tomorrow to give some decision about the priority of AI.¹⁰⁰

Burcham's mention of further klystron problems shows that the visit of Oliphant and the Birmingham klystron system had not at this point led to any improvement. This lends weight to the possibility that the klystron was not even fully understood by the Birmingham team if they were unable to assist the AMRE team to make their work.

Now that he was (unofficially, at least) in charge of the 10cm project, Dee's frustration against the perceived bureaucratic forces ranged against him began to mount. Being in charge of the project led him to be exposed to higher levels of authority than he had experienced previously, as described in the following passage in his diary:

There has been a lot of talk about the impracticability of centimetres due to specular reflection. All the defeatists quote this as a reason for not driving for centimetres. Even at high level meetings in London between people who have never seen a centimetre valve these objections are being aired. Some people believe that this is likely to make the pick up very uncertain.

¹⁰⁰ Burcham (unpubl.) 12/7/40, from Burcham/Lovell Correspondence Dec. 1990.

This is an extremely significant quote. The main reason that the Germans failed to pursue centimetre waves was on the basis of experiments conducted in 1939¹⁰¹ which led them to believe that the reflections from aircraft illuminated with centimetre radiation would be too weak to detect. Their experiments led them to construct a theoretical explanation that confirmed this view, and once established, this view informed all further experimental work until the arrival of the British H₂S. Even after the discovery of the ground-mapping radar, they failed to believe that centimetre waves could be used in AI until the recovery of a British Mark VIII centimetre AI.

The distance of the officials from the experiments at AMRE led to a conflict between their theoretical beliefs about centimetres, and Dee's beliefs informed by his experience with working with them. The AMRE team's practical knowledge of what they could actually do led them to challenge the view that centimetre waves were useless. It would have been quite possible that the "official" view would have been the one that pertained, as happened in Germany where authority carried a lot of weight. However, rather than just accept these "facts", the AMRE group decided to investigate for themselves as the remainder of the quote from Dee's diary illustrates:

We are therefore doing a rough polar-diagram of a Lysander with 50 watts of 8cm C.W. and 6' mirrors, as was pretty obvious the aircraft had a pretty peaky polar diagram but at all aspects there seems to be a fair return. Hope this will allay the boneheads at the top. Had to write this result formally on a file for the Chief Superintendent [Rowe] to read!¹⁰²

Dee recorded the results of the experiments in his diary in a very interesting fashion, away from standard scientific convention. Using the split-anode magnetron that Lovell had for testing parabolas, they mapped the reflection pattern from a Lysander aircraft. The "peaky" pattern refers to there being more energy reflected at certain angles than others, for example the side offered a bigger reflective surface than the front. However, "there seems to be a fair return" is not the sort of language one would expect in a report or paper. This indicates the still uncertain nature of their beliefs about centimetre radiation at this stage. Furthermore, that he should make special mention of having to

¹⁰¹ See chapter 6 for more details.

¹⁰² Dee (unpubl.) 15/7/40.

write up results formally indicates that it would not have been a usual practice. This was also confirmed by Burcham, who in interview said, amongst other things, that it was “an effort to write anything”. He also said that there were no progress reports in general; brief notes were written down for personal use, as aides-memoire and to develop ideas in a graphical form (figure 4.6, from Burcham’s notebook, also illustrates this). Equipment got improved through trial and error, as they conducted experiments.¹⁰³ Their mode of working was one of close, informal co-operation in a highly practical environment. Modifications and understanding about centimetre waves became embodied into the researchers as they performed their experiments.

By mid July, 2 months after the arrival of the team members, the structure of the group was finally beginning to emerge. At the same time the cavity magnetron arrived at Worth Matravers. The arrival of this valve altered the speed of progress significantly. They had hitherto managed only to perform some useful experiments to increase their understanding of the behaviour of waveguides, parabolas and other centimetre components. Their failure to stabilise the performance of the klystron so that it gave them a regular supply of high-power, frequency-stable centimetre waves had significantly retarded their development of a complete system. They could not set about combining the components into a complete, *demonstrable* system if they were not able to operate one of the key components consistently and to order.

4.4.2 The Magnetron Arrives - The Initial Success on 10cm

On 20th July 1940 Dee noted that they received the GEC magnetron (referring to the E1189 sample), and that he and Atkinson fixed up a modulator to drive it. Burcham noted that “magnetron imminent” on July 18th, and on July 21st he mentioned using it. However, he recorded it as coming on Friday 19th July, and that it was brought down by Atkinson.¹⁰⁴ Batt wrote that on 19th July Atkinson cleared bench space for “a new and exciting toy”.¹⁰⁵

¹⁰³ Burcham interview 15/2/93.

¹⁰⁴ Burcham (unpubl.), 18-21/7/40.

¹⁰⁵ Batt (1991), p50.

The magnetron had an immediate effect on the 10cm group. All the work done at GEC by Megaw had made it into a device that was far simpler to operate than the klystron, which was still prone to stop without warning. Megaw's embodied skill had been embedded into the cavity magnetron, which did not now require the same level of skill to operate as when it had been in Randall and Boot's laboratory. By contrast, the klystron was not fully understood, the AMRE scientists did not have the embodied skill to operate it, and they could not therefore embed their skill into an apparatus that could be replicated and then operated by people lacking the embodied skill of the original researcher(s). With the new device, the AMRE researchers neither had to learn to produce the effect¹⁰⁶ as did Randall and Boot, or replicate¹⁰⁷ the device (as this had been done by Megaw). They only had to "switch it on."

The small, neat, compact magnetron, "the size and shape of a pill box", was fixed up in a wooden cradle between the poles of a permanent horseshoe magnet. Cooling air was supplied by a large blower. The high-voltage supply for the klystron was used for the magnetron. Batt described what happened when the set up was completed:

With the precious magnetron in position and connected up, Jimmy [Atkinson] began to wind up the high-voltage supply. One thousand, two thousand ... nine thousand, ten ... Suddenly a neon lamp being held against the output stub of the magnetron started to glow: the magnetron was oscillating, producing power - 10 centimetre power! It was all so simple, unbelievably simple. We didn't audibly cheer but nevertheless that was the feeling we all shared.¹⁰⁸

They now had a power supply that was stable. It was one less thing that they had to learn *how* to operate.

On 2nd August Burcham noted in his diary that "[t]he magnetron is now ready to be put outside for reflection experiments. I am supposed to be in charge of this although whether Atkinson will let me be, I don't really know."¹⁰⁹ In the two weeks prior to this,

¹⁰⁶ Compare to Gooding's account of how Faraday learned to operate the rotation motor by trial and error experimentation - see Gooding (1990), ch 6.

¹⁰⁷ Megaw was able to replicate the cavity magnetron as he had learned how to build and operate it in the first place. Part of his learning process had involved him making a trip to Birmingham to meet Randall and Boot. This follows the replication process, whereby personal interaction is required, as laid down by Collins (1985), ch 3.

¹⁰⁸ Batt (1991), p51.

¹⁰⁹ Burcham (unpubl.), 2/8/40.

Lovell had moved on to measuring the transparency of perspex to radiowaves, as Dee recorded on 22nd July. Lovell himself recorded that he had got:

Very good results with the 3' paraboloid. Side lobes too small to measure with the available power and marvellous beam shifting $\pm 25^\circ$ with a shift of 10cms and the diagram not gone too badly to pieces.¹¹⁰

After these satisfactory results Lovell was now starting to think of using a beam-shifting method rather than a movable antenna. On 29th July Dee recorded discussions with Gossling and Paterson from GEC concerning the division of effort to produce a working 10cm AI. This issue was now coming to a head, but would not be resolved for another month.

In Dee's diary the month of August opens with "First cm echoes in trailer at Worth". This was the month when the team made rapid progress after receiving their magnetron. However, Dee still felt impeded by the system. On August 8th Sir Frank Smith (Head of CVD) visited and was shown "one of the few things well organised". As I have already mentioned, the fact that GEC were making rapid progress forced AMRE to consider how they too could produce a demonstration of an apparatus. For them to demonstrate something successfully, it had to be at the point where they were able to operate it as and when they required. In other words it could not be something like the klystron that was not understood and was prone to "misbehaving."

Smith asked Dee what was required for AIS:

I [Dee] said that all that was necessary was for me to have 10 minutes with someone who had the power to tidy up the appalling muddle and lack of direction of the work.¹¹¹

As Smith was that person, Dee went on to impress upon him the need for further co-ordination of the whole effort between AMRE, the Services and the manufacturers. He saw AMRE as the research *centre*, the hub that connected the RAF who used the device and the manufacturers who made the equipment that AMRE developed.

¹¹⁰ van der Hulst/Lovell Correspondence, Lovell (unpubl.) 22/7/40.

¹¹¹ Dee (unpubl.), 8/8/40.

Despite Dee's difficulties with administration, things were starting improve with the magnetron equipment in early August. They were now at the point where all the various components were ready to be put together into one apparatus in order to start conducting tests. The transmitter and receiver were attached to different, independently mounted paraboloids, and according to Lovell on August 8th and 9th they managed to receive clear echoes from a coastguard hut some 500m distant.¹¹² However Burcham, in his notebook, recorded this as happening on 10th. He also made note of a difficulty:

Running magnetron and looking for reflections. Able to get coastguard hut easily and well but difficult to direct large paraboloid onto a plane. Didn't get it in fact.¹¹³

Burcham's note show that on August 10th they were still not in the position of being able to correlate a signal on their display as coming from an echo from an aeroplane. Such a calibration was very important for them. Over the previous five years, radar researchers had used demonstrations of receiving aircraft echoes as a means to reassure their military superiors that they were spending their money on a worthwhile project. The AMRE teams, facing the challenge of GEC's 25cm system, were now also in the position of having to produce a demonstrable apparatus in order to continue their research.

The episode that follows is very important, as it illustrates several key historical points. Firstly, it shows how Lovell, Burcham and Batt calibrated their equipment so that they were able to demonstrate its capabilities with confidence to visiting dignitaries. Secondly, it shows how Lovell, Batt and Burcham differ in their recollections of what happened, despite consulting their own written records of the events. Their interpretations of what happened vary according to their participatory role in the events. Thirdly, it shows how the misunderstandings and hostility between GEC and AMRE created their own different perspectives on the personalities involved in the events, and highlights the potential for a lack of co-operation that could have had far-reaching effects. I discuss how hostility and suspicion between rival firms and organisations in Germany impeded their research programmes in chapters 6 and 7.

¹¹² Lovell (1991), p41.

¹¹³ Burcham (unpubl.), 10/8/40.

On 12th August Dee was away from Dorset visiting GEC, who he thought “fear[ed] losing control [of the centimetre AI project].” Skinner and Atkinson accompanied Dee to GEC. Paterson, head of GEC at the time, had this to say of the meeting:

“What a day! Lewis, Dee, Skinner, Bartlett, Atkinson, Ward from Swanage to talk AI and means of co-operation. Except for Lewis there was no articulate idea on the subject from their end. They appear to me to be a group of individualists with little experience in team work- except Lewis. Pressed them to have fortnightly meetings with us to review branches of work and detailed programme. Dee was non-committal. I thought it best to put our proposals in writing in a letter to Rowe. I don’t believe they have yet got the idea of sharing responsibility with us.”¹¹⁴

From Paterson’s perspective it was Dee and AMRE who were in the wrong, and being disruptive. GEC had their 25cm system and felt that they should have their achievements recognised. This was not the view of people at AMRE, as confirmed by Burcham who wrote on 13/8/40 that “Dee & Skinner [were] back very depressed from GEC. There is obstructionism there.”¹¹⁵ AMRE needed to be able to demonstrate their achievements if they were to head off the threat of GEC.

In a relatively quiet atmosphere Lovell, Burcham and Batt fixed up their equipment to conduct more trials. In correspondence with Burcham, Lovell quoted from his own diary to confirm with him the correct sequence of events:

Mon. August 12th: Skinner, Dee and Atkinson away so got the double paraboloid swiveller out of the workshops and with Burcham fixed up cables and by 6 p.m. we were trying to get reflections from aircraft. It was really rather exciting and we definitely picked him up.

Tue. August 13th: Dee quite incoherent after meeting with GEC yesterday. News of our echoes would have been good for GEC one imagines. We had quite a successful day picking up a plane in the morning and a tin sheet and a bicycle on the hill, which was amazing considering it should be right in the ground returns. Watson-Watt and Rowe down in the afternoon and suitably impressed. We got a Blenheim over for Watts at 7 p.m. but unfortunately got nothing. Must have been because it was going tail and front on and not across.¹¹⁶

¹¹⁴ Paterson, 12/8/40, in Clayton & Algar (1991).

¹¹⁵ Burcham (unpubl.) 13/8/40.

¹¹⁶ Burcham/Lovell Correspondence, Lovell to Burcham 26/2/90..

Lovell's comment that "we definitely picked him up" is ambiguous - does it refer to Batt or the aeroplane? In his book of 1991, Lovell decided that they "saw" the aeroplane *first*, on August 12th. However, in his 1991 book Batt believed that they were *unable* to see the aeroplane on August 12th, and he had to make his bicycle ride in order for them to pick anything up:

[I]t was early afternoon and a spirit of depression was abroad since once again our efforts had come to nought. What was needed was a moving target that could be called up to order.¹¹⁷

Batt here has highlighted what the main problem was - according to him they were unable to see anything at all with their equipment and required him to cycle with a tin sheet strapped to his bike in order to provide a slow-moving, easy to see target. This would provide ideal calibration for them. He wrote that Lovell said he had received echoes of Batt "up to saturation" after Batt performed his cycle ride.¹¹⁸

According to Batt, the next day, *after* he had made his callibratory cycle-ride that:

Four very dejected physicists arrived back from their meeting at Wembley. It transpired that GEC had won a moral victory. Whilst Dee and Skinner had little in the way of positive results to report, GEC had taken them up on to the roof of the Wembley building where there 25 centimetre system was set up in a hut. Here they were shown an impressive array of echoes from the surrounding topography.¹¹⁹

Batt's quote shows how important it was for AMRE to be able to demonstrate their 10cm as GEC had done. Demonstration provided an effective tool for using in settling the dispute over who should have control of the AI project. However, the previous day's events were to be very useful. Batt then recalled that Lovell mentioned to them that he had clearly seen Batt on his bike the previous day. On 13th, he had to repeat the demonstration by getting on his bike and cycling off so that Lovell could demonstrate the echoes received from Batt. He wrote that he also had to repeat this for several visitors

¹¹⁷ Batt (1991), p54.

¹¹⁸ Batt (1991), p54.

¹¹⁹ Batt (1991), p55.

including men from GEC. He gave the reason why it was him that was used as being because he was a stable, easily “seen” entity when they only had the cumbersome independent paraboloid mounts.

Lovell, of course, described Batt as a “junior assistant”, someone who was not as important, in his eyes, as a scientist. He also recorded in his diary that the twin paraboloid mount came on August 12th, which is in contradiction to Batt’s recollection. This date for the arrival of the twin-mount is supported by the entry in Burcham’s notebook for the 12th:

Aft[ernoon] - Lovell’s double paraboloid arrived, mounted on stand. Set it up, made rough connections everywhere and looked for echoes with Ward’s crystal mixer receiver. Found coastguard hut alright though there is a big ground return & echoes are very wide. Had a plane over and thought we saw it, but couldn’t be sure, seemed to keep jumping in & out. Easily picked up Batt cycling around with a sheet of tin. This is distinct from ground very clearly.¹²⁰

Burcham’s quote, I believe, explains the discrepancy between Batt and Lovell’s accounts. Batt, as the cyclist, over-emphasised his role to the point of excluding them seeing the aircraft at all. This was not the case; they possibly saw an aircraft. Lovell, however, privileged the aircraft as the thing “seen” first. Burcham noted that Batt was far easier to see than the aeroplane, which they could not be sure they had seen. Lovell, after fifty years, favoured the interpretation that he had definitely seen an aeroplane; Batt, that it was him that was seen. Looking at their original notes opens up the ambiguity that existed at the time.¹²¹

What we can also see from all three men’s quotes is that the business of learning to “see” with the apparatus was *not* straightforward. They had to move from observing something fixed (the coastguard hut), to something slow-moving and easy to direct (Batt), to something far less controllable (an aeroplane) and harder to “see”. Lovell was positive about seeing aeroplanes, but Burcham “couldn’t be sure”. Batt thought they couldn’t see them at all. Despite knowing that they could “see” some things, like the hut

¹²⁰ Burcham (unpubl.), 12/8/40.

¹²¹ See Gooding (1993) for further discussion of how scientists re-interpret their original notes at a later date to give the impression that they had a much clearer idea of what they were doing than is apparent from the early notes.

and Batt, even these may have been artefacts of the equipment. Burcham was challenged on this matter the next day:

Osborne says that we were looking at back kicks yesterday and that the real picture is [see picture from Burcham's notebook, overleaf]. The back kicks are due to the receiver saturating. Output stage has to be modified.¹²²

"Seeing" with their equipment, especially their preferred target (aeroplanes) was a difficult business to start with. However, as I have mentioned, they had to have some results in order to keep their project going, because of the threat from both GEC and from more sceptical members of their own establishment. This is where Batt came in as useful: as he was an easily-directed demonstration aid. The necessity of having a good demonstration is confirmed by Atkinson, who wrote that:

[Rowe] could not understand the abundant enthusiasm shown by Dee and Skinner and their complete assurance of success. You must remember that France had just fallen and I think Rowe was under some pressure to concentrate efforts on projects likely to assist against more immediate air attacks - and there were continual rumours of possible invasion.

My impression that the future health of 10cm work was not 100% was confirmed when I ran into John Mercer, whom I had known well at Bawdsey and who worked directly as personal assistant to Rowe. Amongst his duties was the arranging of high-level visits and he told me of a forthcoming visit and said that we had better make certain our echo demonstration worked well. He also bet me £1 - a lot of money in those days - that 10cm would never be used operationally during the war.

My personal opinion is that the quiet but very strong and definite influence of Ben Lewis was a major factor in the group's being given more resources, now that satisfactory echoes had been demonstrated. A second factor was the support of Air-Vice Marshall Joubert of Fighter Command. Joubert came... for a demonstration following one of Rowe's famous Sunday meetings: the demonstration went well much to our relief.¹²³

¹²² Burcham (unpubl.) 13/8/40.

¹²³ Atkinson (1990), pp26-7. Contrast his view of Lewis to those expressed by Bowen in chapter 2.

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See above, both very depressed from GEC.

Ostrom says that we were looking at back ticks
yesterday & that the real future is $\frac{2}{3}$ $\frac{1}{3}$ - 7 -

back 2nd

Continued with modifying equation
around

Down from a high pass
to a valley a good way down

Figure 4.6 Burcham's laboratory notebook, 13/8/40. A diagram of possible valve kickback.

The men engaged in the work believed in the project, and they needed to be able to persuade senior government and Forces officials that they were worth supporting. according to Atkinson, this was done by Lewis.¹²⁴ Now that they were able to say with certainty that they could see huts and persons, they were able to increase their ability to see aircraft over the next few weeks. Their observation of reflections from a Battle aircraft was noted in Dee's diary on August 22nd. This was the day that Epsley and Marris from GEC came to Worth Matravers to view the 10cm system. It was crucial that they made a good impression with a proper demonstration, and Lovell recorded in his diary that:

[Epsley and Marris] very much sobered down by Hodgkin's presentation apparatus. Then in the afternoon we managed to follow a Battle for 2 miles tail on which was magnificent.¹²⁵

This demonstration provided AMRE with enough ammunition to take control of the centimetre AI project. At the subsequent CVD meeting Dee related their success, and GEC were ordered to cancel their 25cm work and to now support AMRE's efforts.

The final major component for AMRE's apparatus to be completed was the reflecting klystron, designed by Sutton of the Admiralty valve workshop at Bristol. It arrived at the end of August. Ward and Batt had struggled to operate the 20cm local oscillator they were using for their receiver experiments, and Sutton's valve was "sheer magic" by comparison. Like the magnetron, it behaved in a much more stable fashion than its predecessor.¹²⁶

At the end of the month (August 26th 1940) Dee fell ill. He was visited by Lovell, Skinner and Atkinson who sought his support for moving the centimetre group to the stables at Leeson House, a nearby Preparatory School that had been requisitioned to provide further accommodation for AMRE. However, both Lovell and Burcham note the move as having already occurred, on August 25th.¹²⁷ On 20th Dee noted that he had

¹²⁴ It is interesting to compare Atkinson's positive portrayal of Lewis to the negative one put forward by Bowen in chapter 2.

¹²⁵ Lovell (1991), p45.

¹²⁶ Batt (1991), p61.

¹²⁷ Burcham (unpubl.), 25/8/40; Lovell (1991), p42.

officially been given control of the centimetre work. This was as a result of his complaint to DCD (Sir George Lee) on August 8th.¹²⁸

Leeson House was a much better location for experimentation than C-Site. It was, at least, a permanent building that could be supplied with the necessary services for their work, such as electricity and water. Moreover, the hill-top site at Worth Matravers was subject to regular air-raid warnings (this was at the height of the Battle of Britain), and Rowe felt that the school, a few miles from the main site, would be less obvious and less prone to attack.¹²⁹

4.5 Winter 1940/Spring 1941

In September Bowen, Cockcroft and Tizard went to the USA with the cavity magnetron and other items to show to the Americans in exchange for their secrets.¹³⁰ At the same time, the centimetre group moved over to Leeson house. The move to Leeson led to a rapid expansion of the centimetre team. Areas of responsibility became more delineated. Dee was put in charge of applications, and Skinner of basic research. Within Dee's group people specialised on different areas. People straight out of University were recruited directly to the team.

In early September a meeting was held between GEC and AMRE at Leeson House. After the August 22nd demonstration of their 10cm system, AMRE now had the upper hand.¹³¹ Dee wrote that they "Made short shrift of their 25cm proposals...". A

¹²⁸ Lovell (1991), p46.

¹²⁹ Batt (1991), p70.

¹³⁰ The "Tizard Mission" was an amazing move on the part of the British Government, effected at a time of great Peril for Britain. Sir Henry Tizard travelled to America with a team of several experts (Bowen being the radar expert) and several top-secret and highly important British inventions, which the British offered to the Americans in exchange for similar knowledge of American secrets. Chief amongst these was the prototype cavity magnetron, which Bowen was tasked with explaining. Once there he liaised with American scientists to set up the Radiation Laboratory at MIT, an institution dedicated to microwave radar research. For details see Bowen (1987) and Clarke (1965).

¹³¹ Certainly this issue, although buried for the duration of the war, had a marked effect on the way AMRE and GEC viewed each other. After the war, Lovell wrote to MAP at their request about GEC's claims in relation to AI. Dated 3/9/45, I reproduce it in full:

"On Monday 12th August the experimental equipment which we had gradually assembled in a trailer at 'C' site at Worth made it's first attempt to obtain reflections from a Battle aircraft. These were thought to be successful but were not conclusive until the next day, August 13th, when among other things the phenomena was demonstrated to Rowe and Watson-Watt. (This was also the day of the famous 'boy on

programme for a 10cm system using a scanning parabola was devised. Hodgkin wrote that "Dee's team [now] worked closely with the GEC".¹³²

However, despite what Hodgkin recalled, the battle over which group had priority was only recently resolved. Initially, co-operation, which eventually became extremely close, was difficult:

Relations between Epsley's [25cm GEC AI] group and the AMRE physicists were at first somewhat uneasy... Epsley felt that the jump from 1.5m to 25cm was quite big enough and was inclined to favour 25 rather than 10cm. On the other hand, Dee and Skinner considered that airborne radar would only really become effective at 10 or 3cm and that it was essential to capitalise on Randall's magnetron.

I would argue, that this situation was to be expected. It would seem only natural that each group would become attached to the fruit of its own labours. However, in the rest of the quote, Hodgkin concluded differently:

The controversy was partly an argument between engineers, who were very much aware of the difficulties of putting a new idea into practice, and the physicists who believed that any technical difficulty could be overcome provided the basis of the invention was theoretically sound. In this respect my training was helpful since both sides felt that a biologist was too ignorant to be committed one way or the other.¹³³

a bicycle' experiment). By August 22nd the Battle was being followed out to a range of 2 miles tail-on and was demonstrated to Epsley and other members of the G.E.C. on that day. By September 3rd we were getting 5 miles on a Blenheim, so you see that all the significant early experiments were, in fact, carried out on 'C' site at Worth [not at Leeson House]...

"As for the equipment, this was... entirely hand-made. It used two 3' paraboloids with home-made dipoles and parasitic reflectors, one for the transmitter and one for the receiver (common T/R came very much later), and was pointed manually at the aircraft through a crude sight. The transmitter used one of the very first G.E.C.-made magnetrons giving only about 2 or 3 kW peak. The modulator again used one of the early G.E.C. hard valves (I have forgotten the type-number, but it was one that always gave trouble, and after two more years was scrapped in favour of the spark-gap modulator in H₂S). The crystal mixer was made by Skinner, and the receiver by Ward, using a Pye I[ntermediate] F[requency] strip. The display equipment and ordinary C.R.T. with range time-base was also made locally.

"... G.E.C... at that time... were concentrating very largely on 25cms. This was the cause of considerable argument and friction between TRE and GEC and it was not until there was a definite understanding that the GEC work was subsidiary to our technical direction that they gave up most of their attempts to initiate 25cms as a first airborne centimetric AI. This was achieved at a meeting at TRE on September 3rd when they were shown the Blenheim up to 5 miles on the trailer equipment. This, combined with the TRE block for 10cms, largely stopped their efforts on 25cms. They did, of course, continue their work on 25cms and several months later we heard it was being used in an experiment at ADRDE."

¹³² Hodgkin (1992), p173.

¹³³ Hodgkin (1992), pp173-4.

The AMRE group may well have believed that their ideas were theoretically sound, but their success was based on their practice. They had learned *how* to use their 10cm equipment successfully by actually performing experiments with it, and modifying it in the light of these experiments. They did not have a solid theoretical appreciation of what they had done. What they did have, was embodied knowledge about how to build 10cm radar acquired through their experimentation. Some of this could be made explicit, and some could be embedded, but these were further processes that they had to undertake if and when required.

However, GEC were also correct in that very often equipment was rushed into service before all the bugs had been ironed out. This was rectified by the setting up of the Post Design Service, a team of people from AMRE (later TRE) who went out to help the services learn about, install, and maintain the equipment whilst it was in its initial phases, and help train operators and mechanics in how to use and maintain it.

During Autumn 1940 the group settled into the stables. Work continued on improving the components, which were installed in a trailer located on the lawn of the house. Progress continued, and Burcham noted a reflection from an Anson was seen at 6 1/2 miles on September 19th.¹³⁴

During October work was begun to develop a Naval system following a visit by the Admiralty. On October 21st, Dee noted that the 10cm AI work was subordinated to GL, and that there was further disruption to the 10cm AI programme following demands for advances on centimetre ASV for ships. The main problem with 10cm AI was that of single-aerial working, for the powerful pulse had to be transmitted from the same antenna as was use for receiving the very weak return signal. Therefore care had to be taken to ensure that the delicate electronic components used for receiving were protected from the strong transmitted pulse:

AI is, of course, much more difficult than the other problems in that a single mirror must be used both for transmission and reception and the present methods of protecting the crystal seem to be inadequate. This is certainly the key problem from the AI standpoint.¹³⁵

¹³⁴ Burcham (unpubl.), 19/9/40.

¹³⁵ Dee (unpubl.), 28/10/40.

On November 6th Dee had discussions with Nash & Thompson about the scanning “mirror” designed by Hodgkin. He was under the impression that it would shake itself apart. The next day he noted that they had received a Sutton tube, which was to be used for dual working. He also noted that he was now in control of 10cm AI specifically, as Skinner and Atkinson had been diverted to other projects. November 13th saw him viewing Nash & Thompson’s spiral scanner. On 14th Dee recorded that he intended to persuade GEC to build a helical-scanner in case the spiral model didn’t work.

There was a general meeting on centimetre work chaired by Rowe on November 26th. Dee argued for an expansion of basic centimetre research as 10cm AI was being completely ignored in order to concentrate work on the Naval and GL projects. It was only the visit of Joubert, head of Fighter Command, in December, that restored priority to AI.¹³⁶

However, all was not well with these projects at this time. In an unpublished note from 1940 we are treated to a remarkable admission of the unpredictability at this stage of the equipment with which they were working:

Robinson... is furious as the Atkinson trailer results cannot even be repeated in his trailer. As Atkinson points out every magnetron and local oscillator, ever, have different characteristics and there is a tremendous amount of basic work which is necessary before even a repeatable experimental equipment can be specified. This is very upsetting from the AI standpoint since of course airborne operation of equipment is more difficult and I fear we must be a long way from having proved if it is not even practicable to make a second equipment in AMRE with all the scientific effort available. Lewis is even working Robinson’s trailer to show how it can be done but Robinson is saying that he will no stay more than a day or two before giving up as a bad job.¹³⁷

This paragraph gets to the heart of two issues that I am exploring in this thesis. Firstly, centimetre radar was not a stable, understood entity at the end of 1940, despite six months of experimentation and experience with it. Secondly, even when one *had* been successfully tested, replicating the results on another apparatus was not automatic and required, in Dee’s words “a tremendous amount of basic work”. This is confirmed by

¹³⁶ Lovell (1991), p55.

¹³⁷ Dee (unpubl.), 29/11/40.

Hodgkin, who describes the third trailer that was equipped in order to set up a project to do gunlaying:

The third trailer was equipped a little later in order to demonstrate the potential of 10cm radar for controlling anti-aircraft guns... [T]he gun-laying application looked easier than centimetre AI - no serious weight or size restrictions, no absolute necessity to transmit and receive on the same aerial - and so on. But of course the project had its own massive difficulties and work in that trailer went on for many months instead of a few weeks as had been hoped for originally.¹³⁸

What these quotes illustrate, is that at the end of 1940 and in to 1941, construction of a centimetre radar was only partially realised. Lovell, Batt, Atkinson, and most of the other participants give the impression in their biographical accounts that the 1940 work produced a successful radar. However, when one examines original accounts written at the time, a different picture emerges. The team had had some success in managing to produce convincing demonstrations *for others*. They had also learned to produce results with their equipment. However, they were not in the position of fully understanding their apparatus, and being able to replicate the equipment and the results. The participants have reconstructed the uncertain, contingent nature of their apparatus and results of this period, into something far more stable than it actually was at the time.¹³⁹ It required nearly a year's further work before 10cm AI was stable enough to replicate. During this period the researchers increased the level of their embodied skill in operating and building the equipment. They were able to take this skill and embed it into the production apparatus that was produced in mid to late 1942.

4.5.1 AI experiments

Late in the year the experiments commenced on mounting the set into an aeroplane. On December 16th Dee recorded that the spiral scanner designed by Hodgkin was fitted to a Blenheim aircraft. Joubert (head of Fighter Command) complained of the lack of

¹³⁸ Hodgkin (1992), pp169-70.

¹³⁹ For an account of the reconstructive processes that scientists use to make their apparatus and results stable, see Gooding (1990), ch1.

effort going into 10cm AI on 22nd December, and this was when the project really got underway again..

Dee went to de Havilland's on December 30th. They had just had their private venture wooden aircraft, the Mosquito, approved, and one of the uses for it was going to be as a night-fighter. He also noted problems with the single-working aerial, that mixer crystals were being burned out through leakage from the transmitter into the receiver. During January and February 1941 experiments were carried out with the two trailer systems, and one was chosen from these trials, as Dee recorded.

In March 1941 Dee wrote:

[C]rystals slowly die... Main remaining worries - better crystal protection or abandonment of crystal for V[elocity] M[odulated] mixer. Schedule of units drawn up with GEC to permit a crash programme - but many shaky points still.¹⁴⁰

The first 10cm AI flight in the Blenheim occurred on 9th March 1941. The fuses in the system blew, but there was a very unexpected result from flying the system in the air. The display screen also showed a very clear horizon line, which allowed use of the system as an artificial horizon. This could be very useful at night as it instantly gave the pilots an indication of their attitude in relation to the ground. The next day Dee felt able to report that whilst flying with the system they "Saw Battle [aircraft] at 2 1/2 miles for [height] of 2 miles, ie OK." Because of their experience of the ground system, by this time they were much more able to clearly link echoes with known physical objects.

In April they were informed by Birmingham that a 50% efficiency of the magnetron (ie power in to power out) was possible by using a higher magnetic field strength. A new mixer was still required as the old crystal one still gave trouble. On the 2nd April Dee noted that GEC were to make 6 AIS units.¹⁴¹

A new mixer was installed in May, and in June Dee wrote "Common T[ransmit]/R[ecieve] at last."¹⁴² This was the problem of transmitting and receiving through the same aerial without damaging the delicate receiver components by leaking the powerful transmitted pulse into the receiver. During this month he also wrote a

¹⁴⁰ Dee (unpubl.), Feb/March 1941.

¹⁴¹ Dee (unpubl.), 2/4/41.

¹⁴² Dee (unpubl.), June 1941.

report on the state of 10cm AI. This document was to become the basis of the AMRE/TRE Post Design Service, whereby Establishment personnel were formed into a unit which helped the Services adopt the new devices they designed. This service was especially useful with H₂S.

At the end of June, the Oxford valve laboratory proposed the use of the Sutton reflex-klystron valve as a spark-gap in AIS. This was adopted.¹⁴³ On 20th July it was decided that four aircraft would be fitted with experimental sets, and there would be eight spares. In October the first strapped magnetron arrived. Work had also started on 3cm. The strapped magnetrons were much more stable and the GEC crystals were far less prone to burning out.

Late in 1941 work on centimetre radar was much more advanced and better understood than the previous year. Flight trials were being conducted, and there were links between AMRE and companies such as GEC, Nash & Thompson, and EMI. 15 months on from the initial experiments, centimetre radar was far better understood, and there was a small but growing team of men skilled in its building and operation.

4.6 Conclusion

In this chapter I have described the work that went into building the first centimetre radar. I have told what the reasons for and against investigating this area were, and the applications that were envisaged by developing centimetre systems. I have also highlighted the aspects of this story that support my analysis in chapter 1. 10cm radar was developed through the experimental skill of the researchers. It was a practical achievement, rather than a theoretical one. By experimenting with 10cm radar, the researchers acquired embodied knowledge about how to successfully build suitable equipment. This was acquired by trial and error experimentation, and the process was not complete even 18 months after the original experiments.

I hope I have made clear that what they actually had to do was not at all as straightforward for the people doing the research as it might appear to them, and us, afterwards. There were considerable external pressures from, amongst others, Dee's

¹⁴³ Dee (unpubl.), end June 1941.

“boneheads” (the officials at the Air Ministry), for it to be dropped if nothing in the way of results were forthcoming. Therefore, it was very important that the researchers had some means of demonstrating that they had made some progress towards their aim. They did this by producing a ground-based system that could be used to demonstrate that they were able to receive echoes from external objects such as huts, men on bicycles and aeroplanes.

I also hope that I have shown that the development of radar was very similar to the accounts I have given of the first airborne radar (in chapter 2), and the components used in centimetre radar (in chapter 3). It was exploratory, practically oriented work that depended to a large part for its success on the interactions between the team members and their equipment, and the team members with each other. Continued experimentation led to a greater degree of success in learning how to use centimetre radar firstly on the ground, and later in the air.

In the next chapter I describe the development of the H₂S ground mapping radar. Work commenced in December 1941, and it was built using the components and experience gained in building AI. Some of the work was done in parallel. However, as I will show, despite the settled and stable nature of the majority of centimetre components by this time, H₂S was still a very difficult piece of apparatus to make work. Its eventual success depended to some measure on a redefinition of what that success meant in order for it to be accepted.

Chapter 5: H₂S Navigation Radar

5.1 Introduction

In the previous three chapters I have explained how microwave radar was developed in Britain. In particular I have concentrated on the aspects of development that were related to the embodiment of practical experimental skill of the people doing the work, and the embedding of that skill into the apparatus they designed that allowed it to be operated by others not equipped with this embodied skill. In this chapter I shall trace the political and technical events that led to the development of the H₂S radar, and analyse these events in relation to the types of skill I outlined in chapter 1, and above. H₂S was a ground-mapping radar fitted into the four-engined aircraft of Bomber-Command. It was used primarily for navigation and target-recognition, and was introduced into service in 1943.

I shall begin by describing the political and military climate in 1941. It was this that led to an investigation into the whole nature of the British bombing campaign against Germany. This investigation was instigated by F.A.Lindemann, scientific advisor to the Prime-Minister, Churchill. During the summer of this year Churchill was involved in a struggle in the Cabinet over the allocation of resources to the War Effort. This struggle was over the need to address the threat that German U-boats posed to British supply routes, and the feeling that the British should be doing something to carry the war to Germany at a time when Britain was everywhere on the defensive.

At AMRE, now renamed TRE (Telecommunications Research Establishment; an attempt to disguise the nature of their work), staff were developing two bombing aids, GEE (for Grid System) and OBOE (Overlay Bombing Over Europe). Work continued on these two projects under their respective inventors, R.J.Dippy and A.H.Reeves, despite at best indifference and at worst hostility to their efforts. When the results of Lindemann's investigation became known, resources were made available for these two devices. They were installed into bombing aircraft and used as part of a new strategy in

Finally I want to show that the inability of the researchers to build a set that functioned to the requirement of the Air Ministry and the government led to changes in the definition of that requirement, and also to other changes. In other words, instead of modifying the apparatus to perform according to the project specifications, the specifications were modified to fit with what had been achieved with the apparatus. H₂S was supposed to draw a map-like picture of the ground that would enable a navigator to fly in conditions of no visibility without recourse to dead-reckoning methods. Often this was done by comparison with maps. However, in practice it was found that the H₂S picture did not correspond to the map images, and so the maps were changed to fit the H₂S picture. This raises the interesting point that representations, as maps are, do not always represent in quite the way that they are thought to.

5.2 Political Background

After the fall of France and the Battle of Britain in the summer of 1940, Germany began a programme of night-bombing of British cities. The Germans were well able to undertake this task because they had developed a number of radio-based aids to navigation and target-finding at night (see chapter 6). One of the consequences of this action was that the Cabinet were more prepared to give a sympathetic hearing to the Bomber Command staff pushing for more resources to be allocated to a bomber offensive against German industrial targets. Despite the primarily defensive stance of British thinking at this time, strategic bombing as an offensive weapon was not a new idea (see chapter 2). The discussions culminated in a directive from the Vice Chief of Air Staff (VCAS) to the C-in-C of Bomber Command, dated 15th January 1941, urging for, amongst other things, the destruction of German synthetic oil production.¹

Before the RAF made any progress towards meeting this directive there was a further change in policy regarding the type of sorties to be undertaken by Bomber

¹ Webster and Frankland (1961) vol 4, pp132-33. Much later on, in October 1944, synthetic oil production was again targeted. By this time bombing accuracy was greatly improved, and the proposition more realistically attainable.

Command. This arose from a directive of Churchill's made in March 1941, giving priority to the Battle of the Atlantic. Shipping losses were rising alarmingly due to the German U-Boat campaign (see figure 2.4), and the bombers were now diverted from industrial targets to mine-laying in German, and German-occupied, ports, and to dockyard bombing.

In Chapter 2 I mentioned the part Lindemann played in agitating for a solution to the British air defence problem. As the war progressed and disaster followed upon disaster for British forces, he became convinced that the only means of bringing about victory was through bombing German cities. He was also extremely sceptical of the rather optimistic claims that returning bomber-crews made regarding the success of their missions.² Being Churchill's Scientific Advisor, he brought his fears up and Churchill authorised him to undertake a survey into the accuracy of British bombing. This was conducted by taking photographs of where the bombs dropped from British aircraft in several raids over July 1941 had fallen.

The results were not impressive. The survey was undertaken by D.M.Butt of the War Cabinet Secretariat, who found that only one crew in five bombed within 5 miles of the target. The number of crews who dropped their bombs within even half a mile of the aiming point was about 5%. As the VCAS had again changed his directive on July 9th back to attacking German cities, it was clear that the RAF had to significantly improve their accuracy to justify even the present level of expenditure on their bombing campaign, let alone expand it to the levels required in Lindemann's plans.

5.2.1 TRE Involvement

Churchill minuted the Chief of the Air Staff on the strength of Butt's conclusions, with the statement that the report "seems to require your most urgent attention."³ The most important result of this discussion was that it prompted a Sunday Soviet meeting at

² See Snow (1961) and Clark (1965) for details.

³ Churchill (1951) vol 4, p250.

TRE, on 26th October 1941, chaired by Rowe, to discuss the problem of bombing accuracy and navigation to and from the target.

According to Rowe⁴ one of the main reasons why there was no requirement for the development of radio aids to navigation and bombing was that there was no appreciation of the way bombing operations would have to be carried out prior to the war. Unlike Fighter Command, which had had several years benefit of reasonably realistic interception exercises, Bomber Command entered 1939 still firmly convinced that Bombers would be able to operate in daylight and navigate by dead reckoning. Even had the realisation been made that this would not be the case, the difficulties associated with locating and accurately bombing well-defended, blacked-out targets many miles away would have been difficult to simulate. Bowen also commented, in private communication to Lovell, that "Dippy was driven to paranoia about the treatment he received on GEE and was still going on about it twenty years later."⁵

At this time there were already two devices under development for Bomber Command by TRE. They were called GEE and OBOE. GEE's inventor, R.J.Dippy, had already spent several years working on his idea without any official encouragement. Through Dippy's perseverance in October 1941 it had reached the production stage. The idea behind GEE was simple: three widely-spaced transmitter stations transmitted pulses and a device in the aircraft measured the time difference between each out station and the centre station. Measuring the time difference between the central station and one of the outside stations would locate the aircraft on a hyperbolic curve. By comparing the time difference between the centre station and two outside stations, the aircraft could be precisely located as it lay on the intersection of the two hyperbolæ. As an aid to speed up navigation, special GEE maps were produced with hyperbolæ marked onto them, corresponding to fixed distances from the transmitting stations (see overleaf for diagram).⁶

Unfortunately, in 1941 GEE still suffered from two problems that prevented it from being brought into operational use. The first is described by Rowe:

⁴ Rowe (1948), pp106-8.

⁵ Bowen to Lovell, 15/5/87, W.B.Lewis Papers.

⁶ For a technical description of GEE, see Dippy (1946).

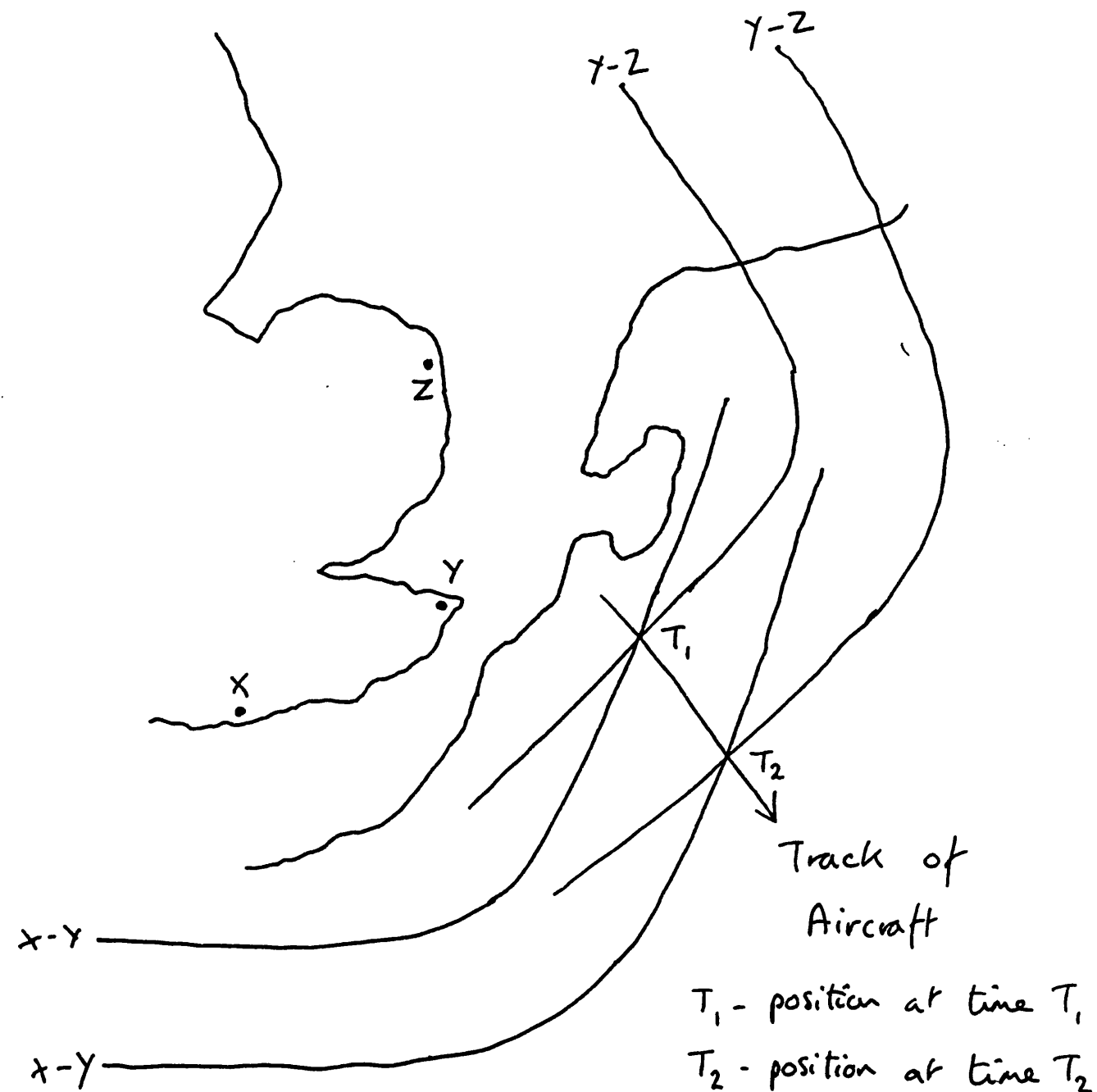


Figure 5.1: GEE. Two stations, x and y transmit a coded signal. The navigator in the aircraft measures the distance from each station and places them on a parabola. Employing a similar technique, he locates them on another parabola from stations y and z . The intersection of the two parabolas is their position.

[T]here was a grave stumbling-block to the immediate use of GEE [as] there were not enough sets. It is commonly supposed that in the 1914-18 war our immediate use of the first few available tanks enabled the enemy to prepare for their use on a larger scale and to start their manufacture for use against us, thus robbing us of much of their value. The timing of the use of a new weapon in relation to the quantity available is particularly important for radio devices because most of them, in time, can be countered by jamming. Bitter arguments on the timing of the use of a new device were not infrequent until the war was nearing its end... Certainly it was right, in the summer of 1941, to await the production of several hundred GEE sets before using the device over enemy territory.⁷

The other problem with GEE was that, due to the curvature of the Earth, its range extended only to some 300 miles. This was enough to target the Ruhr, in the West of Germany, but not enough to target towns further East, such as Berlin, which was 800 miles away. However, GEE offered improvements to long-range navigation as at least pilots would be 300 miles further towards their target before having to rely on dead-reckoning.⁸

The other device, OBOE, was much more accurate than GEE, but its use was even more limited. Because of these limitations it was never really encouraged, and only proved its usefulness by the time it was operational. Rowe described it as "the joker in the pack of TRE devices"⁹. OBOE worked by guiding an aircraft on a circular path which passed directly over the target. A second transmitter transmitted a signal in a circle whose circumference intersected the path of the aircraft's circle at the release point - thus ensuing very accurate release (within 120 yards of the target). The main problem

⁷ Rowe (1948), p110. This was probably the reason that Dippy suffered "paranoia" over GEE (see footnote 4 above).

⁸ Saward (1959), p61. Dead-reckoning required a complicated series of calculations based on the position of the aircraft relative to its starting point. This involved calculating its track in relation to its motion in three dimensions, this motion being dependent on variables such as wind speed, height, evasive action taken, fuel and bomb-load etc. If visual fixes were unobtainable navigation became a very hit-and-miss affair, and unless navigators were highly skilled the target could be missed by tens of miles quite easily. Wind speed in particular was always a difficulty because accurate forecasting relied on the ability to measure it on the way to the target at the time of the raid, something that could not be readily accomplished in wartime.

⁹ Rowe (1948), p112.

was that only one aircraft could be controlled every 10 minutes, and that its range was again only enough to reach the Ruhr. When OBOE was eventually introduced into service in December 1942, it was used to mark targets with considerable success, but could not be used by individual aircraft (see overleaf for diagram).¹⁰

5.2.2 The "Sunday Soviet" of 26/10/41

When, in August 1941, Lindemann was made aware of the contents of Butt's bombing-accuracy report, he immediately agitated for something to be done. Lindemann, along with others such as Harris, had long believed in the necessity of having a large bomber force. He believed that strategic bombing of cities, thereby destroying factories, homes and workers' morale, was the only way to defeat Germany. The other major effect this would have on German civilians, that of killing them in large numbers, was not openly mentioned. Even if strategic bombing did not bring about direct defeat, Lindemann believed it was an essential prelude to victory. However, in 1941 Bomber Command was both too small and its methods were too inaccurate to execute such a task.¹¹

The current head of Bomber Command argued that Butt's report was too pessimistic, but Lindemann insisted to Churchill that even if it was an exaggerated picture, things were still bad enough to warrant serious attention. Surprisingly, TRE too were unaware of how bad things were. Rowe thought that everyone (excluding those of Cabinet rank) were deliberately kept in the dark, probably because if the information were made public, it would have been nigh-on impossible to send bomber crews out night after night if their missions were shown to be largely pointless.¹²

Prompted by Lindemann, Churchill minuted the Chief of the Air Staff of the report's findings on September 3rd 1941. Now that the Prime Minister was aware of the

¹⁰ Saward (1959), pp108-14. For a technical description, see Jones, F.E. (1946). Jones was Reeves' assistant at TRE.

¹¹ Saward (1984), p112.

¹² Rowe (1948), p115.

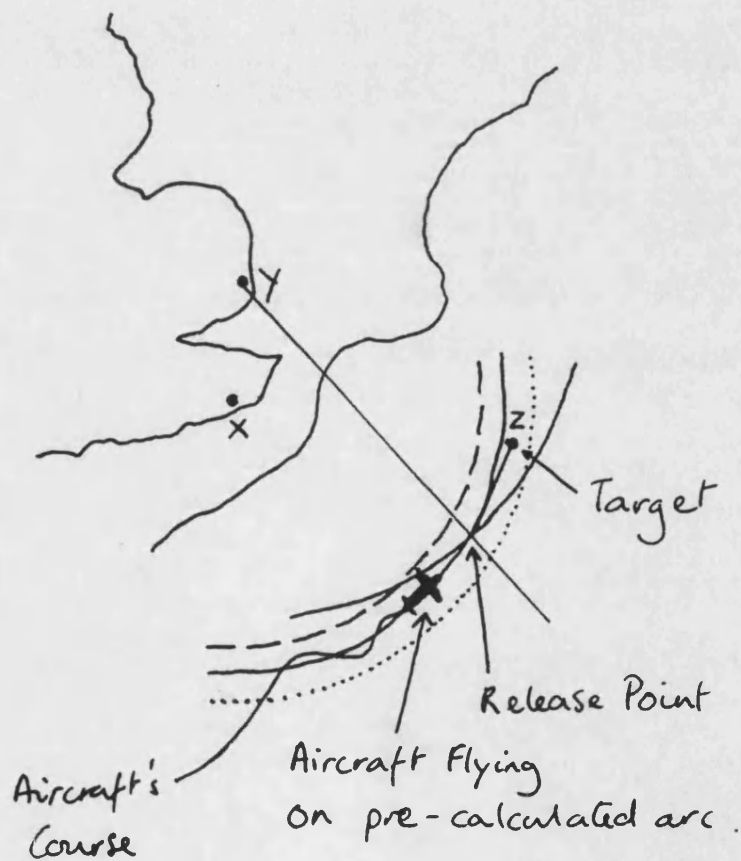


Figure 5.2: OBOE. The first transmitter station *x* transmits a signal in an arc, such that nearer to the station produces a “dots” tone in a receiver, and further away produces a “dashes” tone. The signal is calibrated so that when the aircraft flies on a particular arc, a continuous tone is heard (dots + dashes). This arc passes in such a way that when transmitter *y* transmits a signal at a predetermined point, the bombs are released to fall on target *z*.

situation in Bomber Command, Lindemann engineered the appointment of Sir Robert Renwick to co-ordinate the production of radio aids for bomber aircraft. Renwick was also in charge of the production of four-engined bombers at MAP, so the two appointments tied in well.

Rowe called a Sunday Soviet meeting at TRE for 26th October 1941. Precisely how it came to be called is not clear, but it was probably pressure from both Lindemann and Renwick¹³ that prompted Rowe to arrange the meeting. Rowe wrote that the purpose of the meeting was to discuss ways "to help Bomber Command bomb unseen targets."¹⁴

Lindemann was very keen that all of Germany should be within reach of Bomber Command, so GEE and OBOE were ruled out of bounds for this particular discussion. No minutes were taken of these discussions, so the exact details of what was discussed are not known. Writing in 1948, Rowe reported that they:

...discussed the possibility of self-sufficient equipment [that] might enable electric power lines to be followed or which might detect towns by virtue of the magnetic field associated with the electrical installations... I recall that we went to our homes without an idea.¹⁵

Lovell, who was later to be in charge of the project, was not present at this meeting. As he recalled in 1991:

[I]t was customary for Rowe to summon only the senior scientists who had interests related to the subject under discussion. In any event the most significant TRE person present on that day was Dee whose diary entry reads only: 'Big VCCE meeting/Soviet at TRE on how to locate targets'.¹⁶

¹³ Lovell (1991), p87.

¹⁴ Rowe (1948), p117.

¹⁵ Rowe (1948), p116.

¹⁶ Lovell (1991), p87.

That the meeting ended without any ideas is puzzling - as Lovell pointed out: "Although the ground return echoes on the metre wave AI systems had been a serious trouble for air interception, their potentiality for navigation had been recognised before the war by Bowen and his group."¹⁷

Bowen and Hanbury-Brown made several flights in early 1939, where they were able to navigate by measuring the height above ground using a 1.5m AI set, and then compare the readings to the contours on a map.¹⁸ Tizard recorded in his diary in early 1939 that "Bowen and all concerned are very keen on using a form of RDF 2 [AI] as an aid to navigation."¹⁹ That these developments were not generally known about by staff at TRE was probably due to the absence of Bowen, as "[he] was in the USA and the core of the original metre-wave airborne group was dispersed."²⁰ Bowen was not in a position to pass on his experience due to his physical absence from TRE.

However, Rowe would probably have been informed by Bowen of the results of his and Hanbury-Brown's test flight. It is odd that Rowe did not mention this at the meeting, except that there is evidence for much personal animosity between Rowe and Bowen (see chapter 2). What impact this may have had on Rowe's recollection of projects is unclear, but it is possible that it may have coloured his judgement of Bowen's results. Lovell recalled in 1991 that Bowen:

...in the winter of 1939... continued his experiments on town identification with metre wave equipment in an Anson, but lack of encouragement and the troubles with the 1 1/2 metre AI caused these experiments to be abandoned.²¹

It is highly likely that as Bowen and Rowe's dispute grew, Rowe ignored anything that Bowen did. The residue of this antipathy may well have led him to omit to mention Bowen's results at the meeting on October 26th, 1941. Within the context of the

¹⁷ Lovell (1991), p88. See also Chapter 2.

¹⁸ Hanbury-Brown (1991), p31.

¹⁹ AP1136, p21. From Tizard diary, 17/2/39.

²⁰ Lovell (1991), p89.

²¹ Lovell (1991), p89.

meeting, the deadlock was broken a few days later, so it did not in the end have a great significance.

This episode illustrates that having persons with embodied skill and knowledge available to be questioned or consulted was very important. I have catalogued some of the instances where experience did play a large part in helping a project move forward, in chapters 2, 3 and 4. In this case, there was a large possibility that no-one would have come up with a solution involving terrain-following. As it happened, this was not the outcome - but there may have been other instances where Bowen's expertise could have helped, that have not come to light. The availability of experienced researchers, with their associated embodied knowledge, was an important resource for the centimetre team.

5.3 The Development of H₂S

5.3.1 The First Town-Detection Experiments

Despite the apparent negative outcome of the meeting on October 26th, just a few days later work commenced on a promising project. There are no records of the meetings at this time, but Lovell has uncovered some evidence of what happened in his research for *Echoes of War*.

At this time, the Blenheim (N^o V6000) aircraft being used for centimetre AI trials was fitted with GEC's helical scanning system. This system was not producing results as good as those obtained with Hodgkin's spiral scanner system. In correspondence with Dr Bernard O'Kane²² whilst writing his book, Lovell uncovered information revealing that Dee probably *did* have prior knowledge of the topic of the Sunday meeting.

²² At this time O'Kane, a member of the GEC research team, was attached to TRE to assist with the helical scanner trials. Lovell (1991), p94. This is a good illustration of how researchers with embodied skills are used to assist those learning how to use new equipment. O'Kane came from GEC, the source of the apparatus.

to O'Kane's diary, on Friday 24th October (i.e. two days before the Sunday Soviet meeting of 26th October):

PID [Dee] came over and stopped our flying as the other 10cm people wanted to fly and he gave them priority. Actually he had a suggestion which he wanted to discuss with us so there were extenuating circumstances. The suggestion involves a break in the AISH [helical scanning AI] programme for a week or so ...²³

Certainly Dee would have been aware of the progress of the centimetre ASV system, which had been undergoing trials since March 1941. This system was based on the AI set also under development (see chapter 4). Experiments with centimetre ASV showed them that it was possible to detect a submarine conning-tower, and that coastlines were also very prominent. Indeed, the detection of coastlines was also a major feature of the 1.5m ASV; similarly, as has been mentioned, the whole centimetre AI programme was *begun* because of the limitations of the 1.5m system caused by ground returns.

All through 1941 the centimetre teams conducted tests from trailers at Leeson House. One of the methods that they used to calibrate their equipment was to fire a pulse at, and receive echoes from, the town of Swanage (Leeson House stands at the top of a slope, which leads down to Swanage some 5 miles distant). Furthermore, Lovell was aware of the way the reflections from Batt on his bicycle (see previous chapter) stood out from the ground as early as August 1940, and made careful note of the surprise that this was. It must have indicated to him that centimetre waves behaved in a particular way in relation to objects and the ground. Perhaps the last word on the meeting of 26th should come from O'Kane's diary, quoted in Lovell's 1991 book:

Why nothing was said ... is a mystery. perhaps Dee feared that the test might be forbidden as an unjustifiable delay in the AI programme. Alternatively he might have wished to have some evidence of a possible solution before raising any hopes.²⁴

²³ Lovell (1991), p92.

²⁴ Lovell (1991), p93.

Nevertheless, the next day O'Kane and his assistant, Hensby, obtained permission from GEC to interrupt the AI test programme, and the AI scanner was modified at the TRE workshops on the 28th to rotate at a fixed angle of depression, rather than rotate and move in the vertical plane to produce the necessary AI coverage.

At this point it is useful to take stock of what knowledge was available to the team about to begin experimenting with town location by centimetre radar. By October 1941 there was a considerable number of people engaged on research in this area. Whilst the behaviour of centimetre waves and the components associated with them was not yet fully understood in all its aspects, there *did* exist a body of practical expertise in building, testing and flying centimetric equipment. This indicated that knowledge of airborne centimetre radar had not yet become embedded. Researchers with embodied knowledge were still required in order to build and use the equipment. This meant it was not in the position of being turned into production equipment.

Many of the ideas that would be used in H₂S were already in the domain of common knowledge to the team. Firstly, the possibilities of using centimetre waves to distinguish objects from background clutter was evident from the ASV trials. Secondly, they all had expertise in building and flying with centimetre systems, although to different degrees and with different areas of specific expertise. Nevertheless, they were no longer novices in this respect. In October, the first strapped²⁵ cavity-magnetrons arrived at TRE, making the stability, and therefore the ease of operation of the sets, much better. Thirdly, the concept of using a rotating scanner and plan-position indicator was already established through its operation in GCI, and also in ASV. What was required was the insight to put them together. Dee appears to have supplied this, prompted by the problems of Bomber Command.

On Saturday November 1st 1941 O'Kane and Hensby made a flight with the modified AI system over Southampton and, as Lovell says, "the radar echoes from the

²⁵ Strapping involved soldering wires to the anode block, to connect different cavities together. This stopped the frequency of the magnetron oscillating in secondary (harmonic) modes, and altering the wavelength of the output.

town were clearly distinguishable from the ground scatter".²⁶ Several more flights were made over Salisbury Plain and the Bristol Channel, and this time photographs were taken of the image on the CRT, a range/azimuth (B-Scope) model (see overleaf for pictures).

The prints were taken to Dee, who immediately showed them to Rowe. Rowe was very excited and thought that this was "the turning point of the war".²⁷ The results were communicated to Lindemann who immediately pushed for resources to be put into this new system. It is interesting to note the very crude nature of the images that these photos represented. Despite Rowe's excitement, there was obviously a lot of work to be done before these crude "splodges" could be made into a navigation aid which required a high degree of accuracy.

Rowe's and Lindemann's reactions were to prove the prologue to the whole story of the development and first operational use of what was to become H₂S. Their excitement at the possibilities belay the difficult task of actually making it work. Often throughout the development of H₂S, the politicians would assume that problems were virtually solved when this was far from the case. What the images being obtained from the display in the Blenheim V6000 *actually represented* was not clear cut. Many different objects stood out, and:

[t]he crucial question was whether radar responses obtained in separate flights were definitely associated with specific ground objects.²⁸

This problem was one that would continue to be associated with the H₂S trials until the following October, a year away. It is another instance of one of the general themes I set out in chapter 1, that of learning to "see." In October 1941, building a device to transmit and receive centimetre waves was, as I have said, unproblematic. What was very problematic was the whole question of the image produced on the screen, and whether that image could be tied to objects on the ground to the extent that a person

²⁶ Lovell (1991), p93.

²⁷ Rowe (1948), p117.

²⁸ Lovell (1991), p94.

interpreting those images could relate them to a map, and so navigate "blind". As Saward explained, writing in 1956:

Th[e] picture was not expected to be a replica of either a map or of the detailed landscape which the eye normally sees. Rather it would be a series of spots of light of varying degrees of brightness which had to be understood, but which would be comparable to the map picture and could be correctly interpreted... The representation in all cases would correspond to the general shape of the objects seen. In particular, the contrast between land and water would be of a high order because land gives an appreciable echo whereas water gives back very little signal.²⁹

Saward captured the essence of the problem of producing a useful aid. In the rest of the chapter I will show how the researchers had to change their definitions of what was an acceptable representation in order to meet the targets set for them by the politicians and the RAF.

Lindemann was extremely enthusiastic about the preliminary results obtained at TRE and his agitation in government circles based on these results produced immediate results. He persuaded the Secretary of State for Air to call a meeting to discuss a policy for developing the new device. This meeting was held on 23rd December 1941, and brought together several persons representing all those with any interest in the development and usage of H₂S. Important amongst these were Lindemann (who was now raised to the peerage as Lord Cherwell), Dee from TRE, and various staff members of departments and services directly connected with producing or using a new electronic device.³⁰

At this meeting, Lindemann raised the issue of whether to develop a device powered by the klystron valve. The DGIS (Director General, Intelligence Service) pointed out that the magnetron was virtually indestructible, that it wasn't known to the enemy, and if captured would assist them in obtaining AI equipment that could be used to counter a Bomber offensive. I believe that the DGIS' assessment was only partially correct, as I

²⁹ Saward (1959), p80. This was written 10 years after the war; at this time the appearance of different geographical features on the screen was unknown.

³⁰ A copy of the minutes of this meeting are contained in AVIA 15/1609, paper 1B.

discuss in chapter 7. As a counter to this argument, Lindemann announced that a klystron equipped set should give a range of 15-20 miles, which would be satisfactory. This was done without any reference TRE staff, who were also present, as to what *they* thought about what was or wasn't possible. The TRE staff were very experienced with using the klystron, and knew what they could achieve with it through their accumulated knowledge. This sort of behaviour by Lindemann caused a lot of friction with TRE personnel amongst others.

This sort of "interference" by Lindemann is often reported by TRE personnel when they write of wartime events. Lindemann had a talent for making enemies. Certainly Dee appears to have been annoyed by this remark, as he is minuted as saying that "there was no adequate evidence for this figure, so a magnetron would be needed. Also, klystrons are not yet available." It likely that previous experience with the temperamentality of klystrons (see chapter 4) influenced Dee not to recommend that they be used in this way.

Discussion then continued on the availability or not of the klystron valve, until Lindemann suggested that H₂S should at first be used simply as a "built-up area" finder, rather than a navigational device. Dee managed to get the committee to agree that as the device was still very experimental, detailed decisions about how best to use it should be left until a later date. After the meeting, the Secretary of State for Air issued a directive that tests be undertaken at TRE to ascertain the limitations of the technique, and these tests were performed by O'Kane and Hensby in early 1942. Lovell was given overall charge of the H₂S³¹ project at TRE, on January 6th 1942.

The H₂S story is very much one of how politicians and scientists interacted. O'Kane's view of how things were at the time is significant, because he believed that they were very far from producing anything useful:

³¹ The name H₂S has several origins, depending on who one reads. It is assumed either to have come from the formula for hydrogen sulphide, Lindemann purported to having said that the idea "stinks for not having been thought of earlier", or from a contraction of Home Sweet Home. At any rate, it replaced the initial codename BN (Blind Navigation), which, correctly, was discarded as being too obvious (Batt recalls that he guessed the meaning of the codename to be Bomber Navigation, which was not far short of the truth).

In retrospect, matters must have been pretty desperate for a few range-azimuth pictures [figure 5.3] to have sold the system. It needed the eye of faith to interpret them and apart from my friend John Dickie of Bomber Command the first "outsider" to do so was Don Bennett [Officer I/C the Pathfinder Force, who were the first to use H₂S operationally] when he came on the scene... We had, I think, one piece of luck. The CRT we were using was not very good and not only "bloomed" on large inputs but was also astigmatic. The result was therefore not a small bright spot but a large bright "sausage". I recall the first PPI being not nearly so easy to interpret.³²

As a result, the researchers had what appeared to be very distinct town-like blobs on their screen. This meant that the test flights seemed to show that there was a lot of promise for the system, and that it was possible to produce a map-like screen for navigating purposes. Their faith was unjustified because certain "erroneous" assumptions were made on the basis of these results, as Lovell wrote in 1945:

During the course of these flights the anxieties about flying the magnetron over enemy territory were already prominent and in view of the ranges on towns [35 miles] given by the equipment in Blenheim V6000 an optimistic assumption was made that the lower-powered klystrons could be used. To test this assumption O'Kane and Hensby made a flight 'with the overall sensitivity reduced to correspond with that obtainable with a klystron. The results showed that range reduction was not sufficient to impair the usefulness of the apparatus' - a conclusion that was soon destined to cause trouble.³³

There are several important things note about these tests. The adapted AI apparatus under test was using a form of scanner and presentation that would not be used in the final version - namely range/azimuth as opposed to PPI, with rotating scanner. Furthermore, the Blenheim could only fly to 10,000 feet, whereas the operating height of the new aircraft for which H₂S was intended was at least 20,000 feet. They were trying to draw conclusions by extrapolation from results gained in conditions very different to

³² O'Kane /Lovell Correspondence, O'Kane to Lovell 5/2/93.

³³ Lovell (1991), p95. Quotation from TRE Report N°12/106, in Lovell's TRE Record.

those that were intended for the new device. They believed that altitude would not be a problem, and as I will show this was not to be the case.

Another of Lindemann's ideas regarding H₂S was his belief that a simplified scanner would allow the device to be pressed into service much quicker. His idea was to dispense with the rotating scanner and use a split aerial for location, similar to that used in AI Mark IV (see chapter 2). The report that was made by O'Kane and Hensby specified that using a fixed split aerial system would be "unlikely to provide successful bombing of a specified target", and that "if the use of a klystron were essential the efficiency of the split aerial system would be severely impaired as the range obtainable even with the magnetron is already near the operational minimum."

The researchers at TRE did not have any hard experimental data that they could use to back up their beliefs. What they did have, though, was considerable "know-how" about using centimetre devices, and I believe that this embodied experience led them to make assumptions about what was and what was not possible. They had all been members of the team who had tried, and failed, to build a successful klystron-powered AI radar eighteen months previously. This experience gave them their "know-how" about klystrons and centimetre airborne radar. They certainly had more experience than Lindemann, and they knew it. Therefore, they were not prepared to accept his interference. Eventually Burcham was given the task of persuading Lindemann that his ideas were not wanted at TRE. He did this by taking him up on a demonstration flight. It was easy for them to arrange a demonstration where the system did not work very well, because that was how it always behaved. This demonstration succeeded, which was fortunate as the H₂S team were already heavily committed to the scanner system.

5.3.2 The Scanner

Immediately after his appointment on January 6th 1942, Lovell began procurement for the design and building of the scanner for the system. As I pointed out in the previous section, the team were already thinking of how best to construct such a system

immediately after the first experiments were conducted in November 1941. The team's knowledge of radar-building in both embodied and embedded forms was, as we have seen in chapter 4, comprehensive. After nearly eighteen months of experiment and trial, most of the practical problems of how to build centimetre AI sets into aeroplanes had been solved (see previous chapter). They had the “know-how” to build airborne radar. This is not to say that all the components were fully understood in terms of *theory*, as a technical report published as late as October 1942 states:

It should be borne on mind that the theory of these magnetrons is incompletely understood, and the mechanism described here is only intended to be a plausible picture of their mode of operation.³⁴

However, the important thing is that although neither the magnetron nor the klystron were well understood *theoretically*, in practice the magnetron was far easier to use. The team already had magnetron-using “know-how”, and the device was stable-enough for them to believe that they were better off using it rather than the klystron.

Just after Christmas 1941, Dee, O’Kane and Hensby went to inspect three new types of aircraft just coming into service with Bomber Command, the Halifax, Lancaster and Stirling. They decided that the Halifax offered more potential for locating a scanner in different parts of the aeroplane. This would allow more variation for the purposes of experimenting with the best place to locate the scanner, and chose this type for trials.

At the outset, Lovell and his new team were trying to reproduce the results that they had already obtained with the AI equipment and scanner with range/azimuth presentation in the Blenheim V6000. Replication, as I have already mentioned in previous chapters, is problematic for scientists. When it doesn’t succeed, the replicating scientists have to try and get the original builders to make explicit the tacit knowledge they have about their apparatus’ construction in order to see what, in their opinion, needs altering or adjusting. It is made even more problematic whenever parts of the original apparatus are changed and modified in the replicated equipment, as the original builder does not have embodied,

³⁴ AVIA26/329.

recoverable tacit knowledge about these variations.³⁵ However, the team were now attempting to do exactly this by using a new scanner and method of presentation, a PPI. As Lovell stated:

It seemed obvious to us that, for operational purposes, all-round-looking was essential and that the presentation should be on a plan position indicator (PPI) with the timebase rotating in synchronism with the scanner so that the navigator was effectively presented with a 'map' of the area over which the aircraft was flying.³⁶

That this decision had already been made, was one of the reasons why TRE were so keen to 'kill' Lindemann's idea for using a split aerial system similar to that used in metric AI, with a simple range/azimuth presentation. Lovell and his team had a better "feel" for what they thought would be most usable *in practice*, because they had radar building *know-how* that Lindemann did not.

The rotating scanner in V6000 limited coverage to +/- 60 degrees due to being mounted in the nose of the aircraft (this was perfectly adequate for the forward-looking AI). Also, the beam given by the full parabolic-sectional 28" scanner dish had a beam width of about 15 degrees. Both these figures were unsuitable for the new type of presentation envisaged by Lovell. They needed to alter the shape of the beam in order to comply with the requirements of providing a signal for a PPI type of presentation. According to Lovell's 1991 recollection, the reasoning of the team went thus: the width of the beam needed to be as narrow as possible, to give the greatest possible definition on the sweep. The range of the beam needed to be broad, to give as great a range as possible on the sweep. Lastly, the strength of the returned signal should be dependent only on the nature of the target and the aircraft's height, and *not* on the range of the target. However, designing a scanner that would produce a beam to meet these requirements would not be straightforward because:

³⁵ See Collins (1985), ch 3.

³⁶ Lovell (1991), p99.

[T]he 'vertical polar diagram' was to be the cause of endless trouble and it was to be many a long day before we ceased to worry about this refinement.³⁷

Many of the units for the new device were already stable entities that had embedded radar-building skill in them (centimetric AI units), but there were still decisions that they had to make concerning the units and components that could have unforeseen effects on the experimental results produced. On January 4th 1942 Lovell went to visit Handley-Page, the manufacturers of the Halifax aircraft selected for trials. The immediate reaction of the designer, Handley-Page, was not favourable. Lovell's proposal was to mount the scanner to the rear underside of the fuselage, instead of a gun-turret, which Handley-Page felt would significantly impair the aircraft's performance. Lovell's response was to tell him that it was far better that fewer bombs be carried if they were actually placed on their target, a view in keeping with the atmosphere in the Cabinet.

Action was certainly swift, for on 6th January Rowe wrote to the Director of Communications Development (DCD), Renwick, requesting that he place contracts with Metropolitan-Vickers for the development and construction of two electrical scanning assemblies, with Nash & Thompson for two hydraulic scanning assemblies, and with Handley-Page for two perspex cupolas. Rowe stated that he would "endeavour to supply complete specifications of the[se] items... a.s.a.p."³⁸ The first cupola-equipped Halifax arrived at the TRE airfield on 28th March 1942.

5.4 Political Pressures

This first aircraft dedicated to the H₂S project arrived three months after the go-ahead was given. Although a long period, given the complex nature of the project, and the time at which it was undertaken, this time-scale was actually quite a remarkable achievement. It was due to the intense political pressure that Churchill put behind the project, at the behest of Lindemann. H₂S differed from previous projects both in this

³⁷ Lovell (1991), p100.

³⁸ AVIA15/1609, 2A.

respect, and also in the way that it was developed. Significantly, a major manufacturer, EMI, was involved from the beginning. This was not the case with many other TRE projects with the exception perhaps of GEC and centimetre AI. Also, the level of liaison between TRE and Bomber Command, and the Air Force in general, was increasing significantly compared to that over the introduction of earlier devices. The introduction of new methods such as Operational Research, where service personnel evaluated prototype equipment, came into the forefront of military thinking at this time. This led to experienced aircrew being involved in the development of new devices from their inception. It had the positive effect of giving a significant input into thoughts about the *actual usage* of the device. There were considerable difficulties with getting less technically-adept people to use the prototypes of 1.5m AI (see chapter 2), and this experience changed the attitudes of the researchers. Political pressure was certainly useful in that it enabled Lovell's team to get speedy access to resources. Unfortunately, no amount of political will could enable them to speed up the progress of their research. On the contrary, it often meant that they were unable to undertake the level of investigation that they would have liked which in the long run led them to the lengthening of timescales as they rectified problems not properly solved earlier.

The background to this pressure arose from Lindemann's enthusiasm for using a strategy of area bombing, if its accuracy could be increased. Early in 1942 he sent a memorandum to Churchill in which he stated that Bomber Command could "de-house" (a euphemistic term covering the policy of area bombing) the majority of German workers in the largest German cities, if the devices under development lived up to their expectations.

There is no doubt that there was backing for H₂S from the highest level. Through his association with Churchill, Lindemann was able to exert considerable influence to prioritise the H₂S project above. Lindemann's influence, and the associated pressure produced an ambivalent attitude towards him from those with whom he was associated. On the one hand, the pressure was difficult to cope with, but on the other it enabled obstructions to be dealt with speedily. For example, when Lovell described in 1991 his

visit to Handley-Page in 1942, he attributed the reason the Halifax was allocated and modified so quickly to Lindemann's influence:

This was the first of many occasions on which no action would have been taken had not the Prime Minister been urged by [Lindemann] to give the highest priority to H₂S. [Lindemann] promised us there would be no delay and there was none.³⁹

Similarly, in conversation with me Lovell described how this priority was exercised in practice through the man in charge of Communications in both the Air Ministry and the Ministry of Aircraft Production, Sir Robert Renwick:

[Renwick] was given overriding priority, and he would phone me every day. If there was any problem, he would get his winger [sic] to do it, and if he couldn't he would just pick up the phone and speak to the Prime-minister. It was just like that. The priority was colossal.⁴⁰

Renwick made an impression on others apart from Lovell. Wing Commander Dudley Saward was appointed to a post at Bomber Command Headquarters in December 1941, as head of a newly created RDF (radar) department. Initially appointed to deal with the introduction of GEE, he soon became involved with H₂S, and through that, with Renwick. He often had cause to deal direct with him, and in doing so use Renwick's influence to get jobs done. As he described him in 1959, Renwick was "A marvellous man... the best invention that's ever been installed at Whitehall."⁴¹

Unfortunately there was a downside to the H₂S programme from all this official interest. Lindemann was pressuring for the operational use of H₂S by July 1942, a date far too soon considering that AI, under development for two years, still wasn't in service.. According to Rowe, there was "a danger that H₂S would suffer from too much limelight and too much haste."⁴² The problem was that Lindemann preferred to see any

³⁹ Lovell (1991), p100.

⁴⁰ Interview between Lovell and the author, 1/12/92.

⁴¹ Saward (1959), p74.

⁴² Rowe (1948), p117.

equipment used as soon as possible, never mind how good it was. The team at TRE believed differently. They felt they needed time to conduct experiments and try out the techniques to enable all the necessary experience to be gathered. This would do two things. Firstly, they would embed all their radar-building skill into the equipment, which would allow it to be manufactured easily. Secondly, by doing this, the device would be reliable, stable, and simple enough to use to enable a relatively unskilled, non-scientist operator to use it. This was very important, as they learned with 1.5m AI and as I discuss in chapter 2. Lovell wrote in 1991 that in April 1942:

I did not know of the acute political pressures that had recently developed and which were to inhibit the appropriate research investigation of how to obtain a satisfactory H₂S picture in a high-flying bomber aircraft.⁴³

This view is confirmed in interview, where Lovell said that "there was just no time to do experiments, particularly on the scanner problem." What was important was that Lovell and his team had to go out and actually *do* experiments; they couldn't wave a wand and make the system work without going through the stages of learning by doing that characterised all the other research they'd done on centimetre applications, and they were well aware of this. However, it was a different matter explaining this to people of Lindemann's and Churchill's rank.

On the more positive side, the formation of Bomber Command's RDF unit under Saward had a beneficial aspect. It meant that right from the beginning representatives of the user had an influence in the design of the new device. In the past the scientific researchers had more or less presented a device to the RAF and expected them to get on with it. Hanbury-Brown experienced considerable difficulties doing this (see chapter 2) with 1.5m AI. The new approach was to prove important in the flight-trials stage.

The important thing to draw from the way in which this particular device evolved, there were pressures operating quite explicitly from "beyond the laboratory". This situation often pertains, as recent work exploding the myth of "pure" science shows,⁴⁴

⁴³ Lovell (1991), p101.

⁴⁴ See, for example, Gooding's discussion of Morpurgo's quark experiments. Gooding (1992).

but in wartime the pressure to bring about closure is much greater. We have seen that this pressure led to decisions being taken as to when a device was "fixed", or well enough understood to be put into operation⁴⁵, at a time in advance of that thought apposite by the people involved in creating it.

5.5 Airborne Trials

5.5.1 The Magnetron/Klystron Dispute - Round 1

At the Secretary of State's meeting on 23rd December 1941 the issue of whether to include the magnetron in plans for H₂S development was first raised. The committee were worried that the magnetron would prove hard to destroy, or would be captured by accident if a self-destruct device failed. This particular issue is important to this thesis, as I am trying to establish whether the Germans *could* have learned anything about British centimetre radar from *only* acquiring a captured magnetron. Collins would argue that this would not be the case, as radar-building knowledge could only be transferred through skilled people.⁴⁶ In his view, the British would only have to worry if the Germans captured one of their scientists. There is some evidence that this was already accepted wisdom in the rush by the Americans to recruit German scientists at the end of the War. However, as I show in chapter 7, this is not my view.

When the decision was made about whether to use the magnetron, it had already had been the heart of several types of radar for over a year. One of these radars (the Type 271 Naval set) was already in service by the end of 1941. The others were the airborne AI and ASV, and the ground-based GL3 and GCI sets.

⁴⁵ The scientists' view that they were always being pressurised by officials contrasts nicely to Don Bennett's view of how scientists behaved: "[Scientists] tend to have preconceived ideas and they then set out to prove these ideas regardless of whether they were right or wrong. There is no doubt that when a scientist is determined to prove something, it is very difficult to prevent him." Bennett (1955), p192. Here Bennett made explicit the frustration of the lay-person who doesn't have the appeal to authority that a scientist has when defending their position.

⁴⁶ Collins (1985).

Using magnetrons in the radars mentioned above was not at odds with this philosophy because of the use to which they were put. The AI and ASV sets would either be used in home airspace, or over the sea, so an aircraft loss would not result in capture of a magnetron, which had proved remarkably difficult to destroy. Explosive devices usually destroyed more of the aircraft than the magnetron. The copper anode block was difficult to hide, and British experts unfamiliar with it were able to reproduce it when presented with the fragments.

The Type 271 was only used at sea, which meant that the chances would be that any magnetron would sink if the vessel it was in was abandoned or hit. The GL and GCI systems were used only within the UK. However, H₂S was essentially an offensive system: usage would entail flights over enemy territory and the consequent risk of an aircraft being shot down and the capture of any secret equipment it carried. The committee realised this immediately, so they discussed the possibilities of using a klystron as transmitter valve instead. They considered this valve to be already within the general public domain, as details of it had already been published (see chapter 3).⁴⁷

In 1942 the Germans had shown no sign of possessing centimetre radar. British monitoring of enemy transmissions revealed no signals on these wavelengths. because of this, some people were cautious to the point of querying the usage of centimetre transmissions at all, as they believed that it would alert the Germans to the possibilities inherent in this area. However, more influential people (such as Tizard) believed that it was better to use the advantage whilst they possessed it, as the Germans may well have been close to success with their own systems and about to use them against the Allies.⁴⁸

By the beginning of 1942, the TRE personnel were all convinced that using the magnetron was the only option for making the system a reality. In 1948 Rowe had this to say on the issue of magnetron capture:

⁴⁷ The Germans did indeed have full knowledge of the klystron valve, but were mistrustful of it - perhaps not surprisingly given British experience with it as a transmitter valve..

⁴⁸ This attitude was absent in the case of Window, the anti-radar jamming method. Both sides discovered that it was possible to blot out radar by dropping strips of foil cut to an appropriate length, a fraction of the radar's wavelengths. Although both sides possessed this knowledge by early 1941, the British were the first to use it in 1943. Its use had been delayed until then by both sides for fear of provoking the other side into crippling retaliation. Both sides had grown very reliant on their radar devices. However, by 1943 pressure to use a method that could help reduce extremely heavy bomber-losses was irresistible.

[W]e urged the use of the magnetron, arguing that it would take two years from the date of capture to use H₂S against us on a useful scale.⁴⁹

It is quite probable that Rowe's recollection of his earlier views owes a great deal to hindsight, as this was almost exactly the amount of time it took the Germans to build prototype centimetre airborne radars, as oppose to reconstruct them, as I relate in chapter 7.

The lack of a decision as to whether to use a magnetron or a klystron at the December 23rd meeting had several effects. The official policy, according to Lovell, at this time was to use a klystron as transmitter. To this end, EMI (who were already main contractors for the centimetre AI) were asked to produce fifty H₂S sets. Representatives from EMI met with the Secretary of State for Air on January 13th 1942. At this meeting they discussed the difficulties of H₂S manufacture. After the klystron/magnetron situation was described to Schoenberg, the head of EMI, he ventured that it would take 3-6 months to develop a klystron to give enough power plus another 4-6 months to tool up for volume manufacture, even if lower priority were given to AI which was already using up all of EMI's production capacity. They decided to develop magnetron and klystron equipments simultaneously; TRE would develop the magnetron equipment and EMI the klystron. Schoenberg asked permission to see Dee's equipment at TRE. This was significant for two reasons. Firstly, that someone as high up as Schoenberg would have to get this sort of permission highlighted the secrecy surrounding the project. Secondly, a face to face meeting would allow Dee to give Schoenberg some insight into the sort of tacit knowledge he would need to acquire for his firm. This would undoubtedly make their job of manufacturing easier, and Dee already believed in this sort of co-operation as I related in the previous chapter.⁵⁰

Unfortunately, klystrons and magnetrons were not interchangeable, as the skilled experimenters knew. Any change in one part of the apparatus would mean a series of

⁴⁹ Rowe (1948), p118.

⁵⁰ AVIA15/1609, 14A.

“knock-on” changes throughout. The difficulties that this situation produced were related by Lovell:

[I]f it had only been a matter of re-designing the AI mark VII units using the magnetron or developing the additional electronic and control units required for the specific H₂S application, the task of manufacturing 50 complete units would not have been a difficult matter for a firm with the great experience of EMI. However... this was not the case. The official directive to ourselves and EMI was that the klystron should be used as the transmitter, and - quite apart from the problem of producing a klystron with adequate pulsed power - this naturally reacted throughout the entire electronic system.⁵¹

The problems that this was likely to cause were noted in other quarters too. The meeting of the H₂S committee on January 13th initiated further results, as a memo of 16th January from Rowe to the DCD at MAP reveals. Rowe had already had discussions with Alan Blumlein, EMI's chief circuit designer, about obtaining assistance with H₂S. Rowe wrote that there was no specific TRE apparatus, so it would be difficult for EMI to build a prototype. However, he suggested that EMI could construct some units, as these would approximate to what was needed. They would also speed up production as EMI would then have experience of building the units, and a design already in place. Rowe related to Blumlein that a comparison of a klystron (9Pk2) from Bristol, tested in a system on the ground, with the results from the low power air tests indicated that the 9Pk2 would work. The Signal School at Bristol could supply experimental klystrons, so EMI would construct klystron units, and TRE the magnetron units. He therefore recommended that DCD should place a contract with EMI to produce TRE-designed airborne trials units. However, he noted the need for research on designing a modulator to drive the 9Pk2 klystron.⁵²

One of the major decisions that the team had to make about the equipment was what shape of scanner would produce the ideal polar diagram for H₂S. There were two issues

⁵¹ Lovell (1991), p107.

⁵² AVIA15/1609, 16X.

here: firstly, what was the sort of polar diagram required, and secondly, when that was worked out, how to produce it from a suitably shaped scanner. The difficulty that they had, was that they were still unsure of what shape of beam they required. The only way they could determine what they required, was by going out and performing experiments to see. However, the shape of scanner that they used would determine the shape of the beam they got, and hence what sort of image they produced. If they got the shape of the scanner right, they would get the correct shape of beam, but as they did not know what shape of beam they required it made it very hard to produce the right shape of scanner from the outset. They had to decide on a shape, and see if it worked in practice.

There is a considerable amount of evidence that, as I have indicated with the cavity magnetron, the *theory* of how to design a scanner to produce the correct beam shape was incompletely understood. Even if it had been possible to calculate theoretically the correct shape required, as the scanners were hand-beaten, and there was no guarantee that they could be manufactured to fit the theoretical design. In practice, the best shape for the scanner was found by trial and error experimentation. Lovell had an idea about the best shape to make the scanner, but he did not yet know whether that design would work. The team had to go through a process of refining their apparatus and design similar to how they had worked on the 1.5m AI and centimetre AI systems previously.⁵³ Lovell's initial reasoning went like this:

In our first effort to achieve an appropriate beam shape with a scanner size which we felt could be reasonably fixed underneath the fuselage of the bomber, we decided to try a section of paraboloid 36 inches in aperture and 18 inches deep. The overall aperture would be close to that of the 28 inch paraboloid carried in Blenheim V6000, and hence the sensitivity should be the same. The greater diameter would give a somewhat improved resolution and we hoped that the 18 inch slice of the paraboloid would give an approximation to the beam shape required in the vertical direction.⁵⁴

⁵³ This process is described by Gooding in relation to Morpurgo's refinement of his quark experiment. See Gooding (1992).

⁵⁴ Lovell (1991), p100.

Lovell had worked with centimetre scanners since October 1939. He had gathered two and a half years of experience of how to design scanners for centimetre radar sets. However, his use of the word “hoped” is significant. The quote illustrates that Lovell was able to use his considerable experience to go in what he believed would be the right direction for building a scanner to do what he wanted. Nevertheless, his experience with scanners so far also indicated that it was highly unlikely that he would get exactly what he required first time. An apparatus would often behave in unexpected ways the first time it was used. Often it failed to work at all, as I have recounted with many of the apparatuses and components I have already described, but sometimes there were unexpected bonuses from this unpredictable behaviour. A good example of a benign unforeseen occurrence was with Hodgkin’s spiral scanning AI display. The first time he operated it in the air, he found that the screen showed the ground-return from the centimetre beam as a horizontal line, parallel to the ground whatever the aircraft’s attitude. This meant that centimetre AI could also be used as an artificial horizon as well as a means of detecting other aircraft.

The other major decisions they had to make referred to how to synchronise the rotating timebase of the PPI with the rotation of the scanner, and at what speed to rotate the scanner. They determined these variables by factors such as the pulsewidth, pulse repetition frequency, maximum range required and maximum power available. Eventually they settled upon 80 rpm as “reasonable”.⁵⁵ Still unsure as to which would be the more suitable type of power for the scanner, Lovell ordered both electrically and hydraulically powered prototypes. Both sorts had been tested for AI, and it seemed wise to Lovell to try out both types for H₂S also. After settling all these design variables, they completed prototype units for the system ready. The units were tested briefly on the ground, to ensure they were wired up properly and produced the correct output. However, it was only in the air that they could be properly tested, so the next stage was to install them in an aircraft and commence flight-trials.

⁵⁵ Lovell (1991), p101.

5.5.2 The First Flights.

After Halifax No. V9977 was delivered on 27th March, Lovell and his team fitted it with their experimental H₂S equipment in order to commence flight trials. These airborne trials were really essential, for it was only when the equipment was airborne that the team would have any idea how well the ground would be represented on the screen, and at what heights these representations would break down. Previous experience with AI had shown them that they could take nothing for granted about how radar would behave in the air (see chapters 2 and 4).

V9977 was equipped with a prototype magnetron-powered system. This was designed and built by TRE using units from AI mark VII, with modifications to the display electronics that were necessary to produce a PPI picture rather than the range/azimuth display of the AI system. It also had a hydraulic-powered scanner built by Nash & Thompson. A second aircraft, Halifax No. R9490, arrived on April 12th and was fitted with the EMI-designed klystron system, and the Metropolitan Vickers electrically operated scanner. Thus they now had two aircraft fitted with all the different sorts of equipment which they wished to trial.

According to Lovell, the aircraft was completely fitted with all the units by the evening of April 16th. However, it would not work at all immediately. They found that the 80 volt alternator that supplied the power for the electrical devices was initially inoperative, and so replaced it with a new unit. On the following morning, with this problem rectified, the equipment was first flown and tried. It was not an immediate success as Lovell recalled writing in 1991:

The equipment worked but very poorly. At 8000 feet altitude the range on towns was only four to five miles and the gaps and fades in the PPI picture implied that there must be something horribly wrong with the polar diagram of the scanner.⁵⁶

⁵⁶ Lovell (1991), p103.

This level of performance was far below that which they had already achieved with the modified AI system in the Blenheim. That the equipment failed to work on its first flight was unsurprising to the team, who were well used to having to redesign and modify equipment when it was first installed in aircraft. Unfortunately for Lovell, the first flight of this experimental system was also chosen as the flight when several important people from Whitehall were to have the system shown to them:

My brief diary note typifies the scene in which we were to work for many months:

... we had DCAS [Deputy Chief of Air Staff], DOR [Director of Operational Research], D of R [Director of Radar], DD of Ops [Deputy Director of Operations] and various others down on the H₂S. Halifax rushed to be ready for them. Flew OK but too many loose ends to demonstrate properly.

The assembly of such high-ranking officers to witness the first airborne tests of an experimental system may seem remarkable. On that April morning it certainly seemed so to me and my diary entry records my irritation with Dee whom I thought should have had the scientific sense and power to prevent this.⁵⁷

Lovell was understandably annoyed that he was asked to demonstrate something that had only just been installed and was still in a very tentative state. He knew after much experience with scientific work in general and radar work in particular that getting any apparatus to work first time is a very rare occurrence. The scientist has to spend time getting to “know” his equipment by adjusting it and operating it to narrow down areas that he doesn’t understand. This involves him building up a reserve of “know how” about its quirks.⁵⁸ Furthermore, by using the term “test” Lovell is actually referring to a trial as opposed to a demonstration. Demonstrations, such as the Batt bicycle ride (see previous chapter) were performed when the equipment and techniques were more stable and easily reproducible, by the skilled personnel. The H₂S equipment was very definitely not at this stage when Lovell was required to show it to the visitors.

⁵⁷ Lovell (1991), p103.

⁵⁸ This know-how is embodied, tacit knowledge of the form first outlined by Polanyi (1959). However, a scientist would not refer to it as such.

The main problem with the H₂S set as it was then, was that the picture that was shown on the screen was very little use. It had gaps and fades in the image that made correlation of what was shown on the screen to what was on the ground, or on a map of the ground, next to impossible. The scientists possessed the knowledge, in the practical sense, to build 10cm electronic units that actually worked. Therefore their inability to produce the required result lay with the scanner, which was the only new and untested piece of equipment. This is borne out in Lovell's TRE Record, a personal scrap-book of his part in the development of H₂S, where he described the polar diagram difficulties of the scanner as "probably caus[ing] more than 75% of all H₂S difficulties."⁵⁹

An account of the development work carried out on the scanner is given in a TRE Report of March 1943. I shall relate what it says here, although some of the experiments recounted leap a little ahead of the story (see overleaf for illustration). Lovell's team performed initial experimentation by mounting the aerial on a trailer. A metal plate was mounted above the scanner to represent the fuselage, and they made measurements of the polar diagram from the scanner. At this stage the results they achieved were promising, as the shape of the polar diagram was close to that which they believed to be necessary for producing a good picture (see above). However, when the scanner was mounted in an aircraft and flown, the results they obtained were very poor and not at all what they expected. They conducted initial tests of the set-up at a flying height of 10000 feet, and found a gap in the display at a range of 9 miles. The testers decided to modify the scanner by adding a wedge to the inside of the mirror. They made the decision to do this for two reasons. Firstly, they didn't have time to have another scanner made. Secondly, their experience with working with AI scanners led them to try this method. They reasoned that the plate would focus part of the beam to go in the direction they wanted (see overleaf for scanner modification).

Unfortunately this modification didn't work either - it set up side lobes to the main beam which produced interference at lower altitudes. The modification also led the testers to believe that the fuselage might be having an effect on the poor picture. When they tested it, another gap appearing in the direction of flight - a major problem, as the

⁵⁹ Lovell TRE Record, p5.

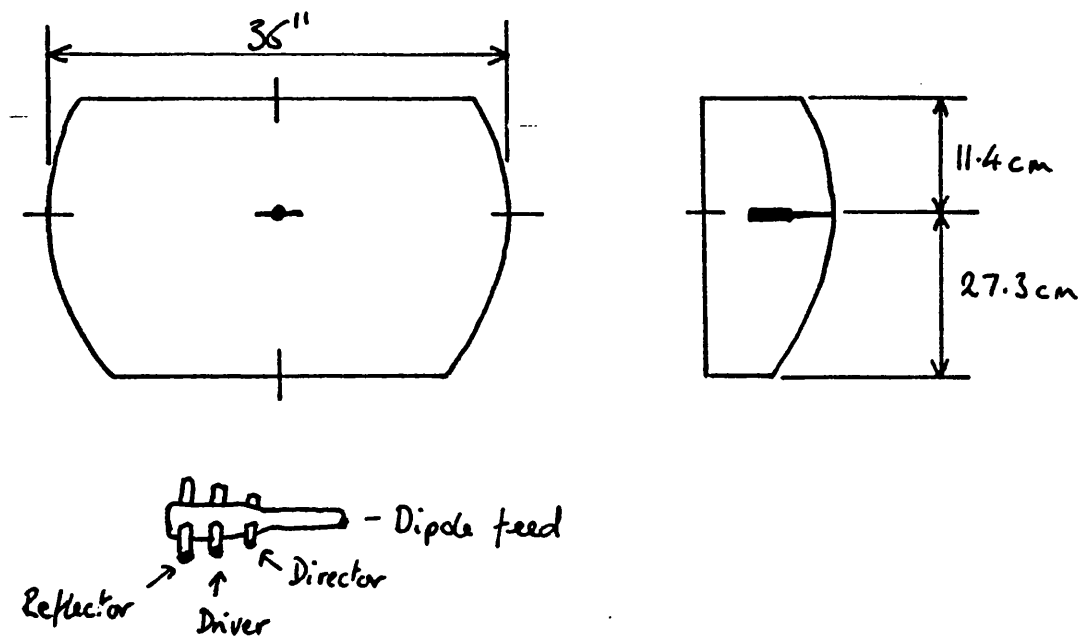


Figure 5.4 The experimental H₂S scanner. Diagram shows the waveguide feed, and the paraboloid dimensions. It was made from a cut-down 36" paraboloid.

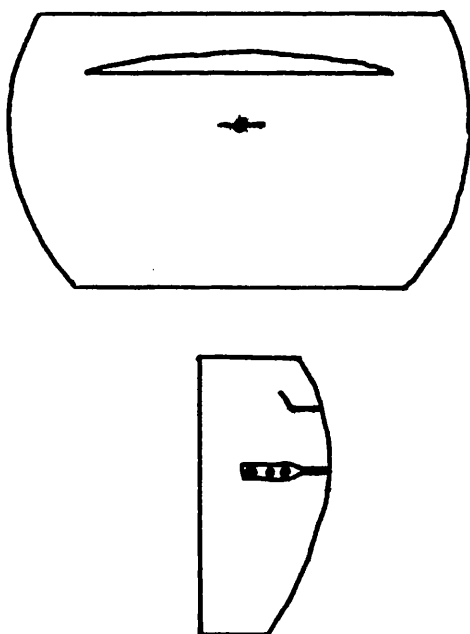


Figure 5.5, 5.5a. The H₂S scanner modification. Diagram, and photograph, showing the bar added to try and cure the gap and fade problem. Photograph from Lovell (1991), p174.

aircraft flies towards a target. A gap in the direction of flight made the target built-up area disappear from the screen. The plate moved the fade from 4-5 mile range to 12-15 mile range, but never eliminated it from this scanner.

During a later flight where they followed an image of Sharpness Bridge from a range of 35 miles to zero gave a fade between 13 and 9 miles range. The testers thought that it might be the fuselage that was causing the interference. However, a subsequent run with the bomb doors (which were ahead of the scanner cupola) open, still had a fade in the same place. To try and combat this effect they added a plywood bar in front of the scanner. This further example of trial and error modification unfortunately also had no effect. It was symptomatic of Lovell's assertion that they were constantly under political pressure to get the system working, without being able to conduct a satisfactory number of tests.

By the time of these later experiments H₂S had already been rushed into service, due to the intense political pressure described earlier. This report is dated 16th March 1943, two months after the first operational use of H₂S, and yet there were still unresolved problems which made its operation very difficult for all but the most skilled operators:⁶⁰

A fundamental problem of H₂S is the general ground return [this caused a bright area in the centre of the display which obliterated any detail at short ranges]. With maximum gain this can be seen out to 12 miles with the present aerial system. This distance seems to be the same for 10000 feet and 20000 feet. By reducing the receiver gain this ground return can be suppressed to allow a built up area to stand out by itself on the tube to within about 4 miles (ground range) of the aircraft position. Contact can be got right in to 2.5 miles ground range but with greater difficulty. If these last few miles could be made more presentable, H₂S operation could be made much easier for service navigators. It is hoped that by a combination of polar diagram

⁶⁰ H₂S was originally fitted to pathfinder aircraft. The pathfinder idea was first tried in the middle of 1942. Highly experienced crews with proven excellence in bombing accuracy were creamed off into separate Squadrons and trained to drop flares and other markers, that the less skilled crews who followed them could aim at. As such, they were already highly skilled at conventional navigation techniques, and so H₂S was originally used by them either to augment their other navigational means, or when conditions were completely uncondusive to ordinary navigation (such as 10/10ths cloud). The pathfinder operators were therefore likely to be the calibre of men who would quickly learn the necessary interpretative skills to utilise the new method effectively, something that was not always the case with other more average men.

measurements and measurements in flight the intensity of the ground return between 0-4 miles can be the same as what is now from 4-12 miles.⁶¹

In 1942 the TRE men had experience of introducing new equipment into service. One of the more common problems they encountered was training operators to interpret the images on the screens. After the first difficulties with AI Marks 1-IV, the researchers gave careful thought to the design of the display. In the case of H₂S, service navigators from the Bomber Development Unit were involved with the TRE scientists from the beginning. Their contribution, as non-scientist representatives of the men who would actually operate the equipment, was to suggest modifications that would simplify the equipment's operation. Sometimes the researchers could not produce the changes requested by the BDU men. This was the case here, and interpretation of the information was always a problem until much later, after H₂S had been in operation for over a year. I shall show some pictures of typical displays and discuss the implications of the images presently.

At the same time (April 1942) that TRE conducted the first magnetron flights in V9977, the team at EMI were constructing their klystron version at their works. Bernard O'Kane, now assigned to TRE, was able to liaise with EMI over problems with H₂S. The EMI team was headed by Alan Blumlein, who had been a pioneer of the first commercially available television sets, and was "regarded as one of the best electronic engineers in the country".⁶² They brought their klystron version of H₂S to TRE on May 14th, where it was fitted into the other Halifax R9490.

The first test flights of H₂S were not a great success. Despite Saward and his assistant Flight Lieutenant John Dickie being enthusiasts for the project, according to Lovell:

[Saye, Saward's Superior] complained bitterly about the 'snowstorm' which is all he claimed to have seen on the PPI and his letter of complaint to the Air Ministry was

⁶¹ AVIA26/482.

⁶² Lovell (1991), p107.

responsible for a meeting summoned by the Assistant Chief of the Air Staff (ACAS) on May 19th.⁶³

This is clear evidence that it took someone skilled in operating H₂S to make any sense of the image on the scanner. The current version of the H₂S prototype was not suitable for demonstrating to sceptical senior RAF officers what the potential of the system was. This meeting cannot have been very supportive of H₂S, as O'Kane recalls it thus:

In my diary I have a note of a meeting (as you [Lovell] were present you no doubt have it too) at which Bomber Command said the system was operationally unacceptable. By that time EMI were well down the road.⁶⁴

Bomber Command may well have been unsatisfied, but the continued pressure from the Cabinet ensured that the momentum of the project was maintained.

At this meeting the operational parameters of H₂S were redefined. This appears to have been an attempt to “save-face” on the project, as it clearly was not performing as well as the concept that had been “sold” to the Air Ministry. the changes to the specification were:

- 2 (a) That the system should be accurate enough to
guarantee that bombs should fall within an
industrial or other selected area as target.
- (b) That the Air Staff would be satisfied in
the first instance if the range of the device
enabled the aircraft to home on a built up
area from 15 miles at 15000 feet.

- 3 Subject to there being no delay or interference
with the development of the equipment and its
introduction into the Service in a form which will
fulfil this aim, it was agreed that details in

⁶³ Lovell (1991), p108.

⁶⁴ O'Kane/Lovell Correspondence, O'Kane to Lovell, 5/2/93.

design to enable it to be used as a navigational aid to determine a specific area or target could be incorporated during later stages of development and operational trial.⁶⁵

Although this directive offered hope, as it now called for a system with less stringent performance characteristics, just four days later Lovell was to write:

[F]lew in the Halifax. Very depressing, picture is extremely bad at the moment.⁶⁶

As if this wasn't bad enough, when the klystron version was first airborne on June 2nd, that too suffered from "gaps, fades and poor range."⁶⁷

By June 1942, after six months of development, the device on which Bomber Command was pinning its hopes was in very poor shape:

- (i) The power source preferred by TRE was the magnetron, but the Cabinet would not sanction it to be used.
- (ii) The klystron-equipped prototype performed very poorly, having too little power to give sufficient range.
- (iii) The magnetron-equipped version performed just as poorly.
- (iv) The image on the screen suffered from gaps and fades, and could not be interpreted well even by the original scientists, let alone RAF service navigators.
- (v) Ad-hoc modifications to the scanner were not improving the picture produced.

The only positive thing for the team was that the specification of the apparatus that they had to produce had been weakened.

The performance of H₂S was far from spectacular. The scientists, engineers and BDU staff who flew with it every day found it difficult to use, and in some cases useless.

⁶⁵ Lovell TRE Record, p5.

⁶⁶ Lovell (1991), p108.

⁶⁷ Lovell (1991), p108.

They had not yet produced a device which *they* could operate, and because of this they were unable to demonstrate it to superiors. They were also under constant pressure to deliver from Lindemann and members of the Cabinet. The prognosis for H₂S was not promising.

5.6 To Operational Use

5.6.1 The Move to Malvern and the Loss of V9977

In early 1942, after an alert Photographic Interpreter noticed a strange “bowl” on a reconnaissance photograph, the British launched a Commando raid against a German radar equipment located on the coast of France. It resulted in the successful capture of pieces of the set, a Würzburg (see chapter 6 for details of German radar equipment). The British did not know what to expect of the equipment, but were naturally very interested to see whether the Germans had matched their advances in the field of centimetre waves. Their monitoring of frequencies had hitherto revealed nothing in the 10cm waveband, or below. TRE staff evaluated the equipment by dismantling the pieces recovered. Amongst other things, they learned that German equipment was well manufactured, but that this particular set worked on 80cm, and was not considered more technically advanced than equivalent British equipment. For the date of its inception (1938) it was superior to the contemporary British set, Chain Home.

The raid also sparked off a fear that TRE, located on the coast, could suffer a reprisal raid. As a consequence moves were made to relocate TRE to somewhere safer. Malvern College School was chosen, as it offered the necessary accommodation, and was located in the English Midlands, as far away from the coast as it was possible to get. The whole establishment moved there during the last week of May 1942.⁶⁸

⁶⁸ The Germans, well aware of the existence of Swanage due to the large Chain Home masts on the site, simply couldn't believe that the British would be so stupid as to put their top secret radar establishment in so accessible a place, and hence left it alone. Pritchard (1989), ch 2.

During May further experimentation with the magnetron system, involving changes to the feed, were beginning to yield slightly better results in terms of the image produced on the scanner. Overall, though, both systems were still very poor. It is important to note that the TRE scientists believed that the problems with the *magnetron* system were soluble, as they had already achieved ranges of 35 miles with the magnetron-equipped modified AI. This meant that they kept faith with their apparatus rather than giving up, although, as O'Kane revealed, they subsequently discovered that the AI-set results were the result of CRT deficiencies rather than being good representations of the ground. By contrast they were not at all convinced about the klystron:

Although the klystron in the EMI prototype was capable of producing a peak power of 5 to 10 kilowatts, none of us believed that the Air Staff directive of 15 miles at 15000 feet could be achieved with that system.⁶⁹

Lovell's comment was written in 1991, so may be the benefit of hindsight. However, contemporary documentation does exist that shows that Dee was against using a klystron-equipped apparatus. The TRE scientists were very experienced with using the klystron, which had never given them as good results as magnetron-equipped apparatus. They did not, therefore, feel that they needed comparative tests to know that the klystron would be a non-starter.

Once the move to Malvern was completed, arrangements were made for the EMI engineers Blumlein, Browne and Blythen to visit in order to see the magnetron equipment (which was designed and built at TRE). They came down on 7th June, and accompanied Hensby and several RAF personnel attached to TRE on a flight over the Wye valley. Lovell and O'Kane were delayed, and couldn't attend. Tragically the aircraft crashed killing all the occupants and destroying the prototype magnetron equipment. As Lovell commented:

⁶⁹ Lovell (1991), p126.

Only the magnetron was recognisable... and it is, perhaps, hardly surprising that I believed this to be the end of the H₂S project. We had lost important members of my small group, the key EMI staff, including the genius of Blumlein, who were to manufacture the equipment, and our only working H₂S equipment which was still far from meeting the Air Staff performance criterion.⁷⁰

This was not the end of the project, despite the loss of many TRE and EMI personnel. Lovell was to reckon without the political pressure which was a feature of H₂S development. Lindemann would not let even this stand in the way of progress, and on the day of the crash, Churchill memoed the Chief of the Air Staff expressing his desire that two squadrons be equipped with H₂S by October. One of the technical implications of the crash was the addition of a height-above-ground tube to the standard display.⁷¹

This galvanised the surviving members of the TRE team into action, and Dee recorded his dissatisfaction with the klystron system at a meeting on June 10th.⁷² Churchill's memo called for a meeting on H₂S a week later, but due to the war situation in Egypt, which had taken a turn for the worse and demanded all his time, the date for this meeting was changed to 3rd July.

5.6.2 The Magnetron/Klystron Dispute Resolved

At this meeting Churchill, wholly convinced of the immediate need for H₂S, bullied the visitors (Dee and Lovell) into the position of having to produce 200 sets by October. Lovell's view was that the meeting was '[at] a level of fantasy that bore little relation to the world outside the cabinet room.'⁷³ The immediate result of this meeting was that Lovell had to rush around the country visiting manufacturers in order to arrange for them to begin production of their experimental H₂S. It was barely off the drawing board, he certainly did not believe it was ready for operational service. However, the

⁷⁰ Lovell (1991), p127.

⁷¹ Musgrove (1976).

⁷² AP1136, p38.

⁷³ Lovell (1991), p135.

intense pressure had some advantages as it allowed him to press the case for discontinuing klystron research. Lindemann came to see their prototype on July 10th, and by July 15th he had persuaded the Chief of the Air Staff to allow TRE to proceed solely with the magnetron version. Burcham performed the task of persuasion. He aided their cause by arguing that replacing the lost klystron-equipped units would delay progress even further.

The next problem was to see whether it was possible to destroy the magnetron mounted in the set. The experiments were not at all successful, usually rendering more damage to the aircraft (which would endanger the crew) than to the valve itself. As the RAF's official history records:

The result of one of the most successful experiments was that a ten-foot hole was blown in the side of a Junkers-88 aircraft, and even then an expert was able to gauge the dimension of the magnetron from its fragments.⁷⁴

It is interesting to note that the RAF mentioned that "an expert was able..." to evaluate the magnetron. Their own experience must have contributed to their fears about what would happen if German experts got hold of a magnetron. It also underlines the whole nature of expertise in radar, as their expert must have been someone who was familiar with electronic devices. This expert must have had practical experience of designing and building valves in order to assess the magnetron fragments.

As it proved impossible to actually *destroy* the magnetron, in September 1942 it was decided to use a small charge to render the valve *unusable* if captured. They then believed that if the valve was captured, its copying was inevitable. The official RAF History records the problems that the committee faced when they permitted magnetron use:

- (i) development of similar installations for its bombers
- (ii) jamming

⁷⁴ AP1136, p40. There is no information as to why a German aircraft was used for these experiments, unless that maybe the fuselage was from a crashed aircraft and British aircraft were repaired if they were in a usable condition.

- (iii) use of decoy targets
- (iv) development of a receiver
- (v) development of a raid tracking organisation to obtain information from H₂S transmissions.

Only the last two were to prove serious problems, although the British could not know this at the time (see chapter 7).⁷⁵

The problems facing Lovell and his team were very large, for, as he recalled, 'a complex of scientific, technological, political, industrial and operational issues were involved.'⁷⁶ Fortunately help was on hand in the form of Renwick (DCD), and Don Bennett, who in July 1942 had just been put in charge of the newly formed Pathfinder Force (see footnote 60)

Bennett came to the TRE airfield, at Defford near Malvern, immediately after the meeting with Churchill on July 3rd. He took up residence in the Mess, and flew every day for a week with the klystron-equipped aircraft. On July 12th this was changed to a magnetron-equipped set. The performance of this equipment was markedly superior to the klystron-equipped set, for Lovell recalls that soon afterwards 'Bennett managed to get a fairly successful magnetron flight'⁷⁷, and became a firm believer in the equipment. A "successful" trial at this time was one that had some sort of "blob" on the screen that could be definitely associated with a town on the ground, at any sort of range and height (see illustration 5.6). At this stage flights were conducted during daylight and calibration could be effected by looking out of the window. His support was helpful in persuading Lindemann to endorse the change to magnetron-equipped sets. The improvement in image-quality that the new magnetron-equipped set produced, happened within the space of a few days. Bennett was a highly skilled pilot and navigator, and it is likely that he would have been able to learn to interpret the images from the scientists very quickly, especially if they had just been improved. He would probably have been able to do this more easily than some for the other, more sceptical, senior officers who were still against the system. This ease must have persuaded him that there was a future in pursuing the

⁷⁵ AP1136, p104.

⁷⁶ Lovell (1991), p137.

⁷⁷ Lovell (1991), p139.

system. Such faith would be especially likely if there was no other foreseeable alternative.

With both Bennett and Renwick firmly behind the project, the 'red-tape' issues involved in meeting the deadline of 50 sets by October became much easier for Lovell to overcome. However, there was one further problem, as Lovell noted in his diary:

Saturday August 8 [1942]. Another Renwick meeting in London yesterday, after another week of struggle.⁷⁸

The struggle, as he notes was that they still had to make H₂S work to the point that they were happy with its performance *and* they could persuade others that towns could be seen using the equipment. They had only had partial success with this latter task so far.

5.6.3 Final Experiments and Operational Service

Fortunately for Lovell the main problems of production involved with meeting Churchill's deadline were largely solved. Almost all the units were already in manufacture for production centimetre AI. His main remaining problem was the scanner, and the difficulties associated with it. Lovell saw the reasons for the lack of success with the scanner as due to their failure to undertake proper ground experiments in order to get the polar diagram of the scanner right before air-testing. When they had used the Blenheim, they achieved good results, but with the Halifax version there were problems as:

...there had been no appreciation of the radical difference between these full paraboloid, narrow beam, clear forward-looking installations and the performance of a truncated paraboloid suffering irregular scattering from the belly of a bomber. In the

⁷⁸ Lovell (1991), p141.

event we were reduced to experimenting with *ad hoc* adjustments to the feed arrangements of the scanner with the bomber airborne at altitudes up to 18000 feet.⁷⁹

These trial-and-error experiments were of a different type to the earlier ones with the wedge. They show that the improvements to the scanner were still being undertaken on a non-theoretical, practical basis.

At the same time as trying to improve the display to their own satisfaction, Lovell and TRE were still trying to convince members of Bomber Command that H₂S was a viable system. The new system was earmarked solely for the Pathfinder Force to begin with. When Pathfinder Force was originally formed, it was done so against the wishes of Harris who firstly believed that competition between his various Groups was good for improving results, and secondly that creaming off the better crews into an elite organisation would be bad for morale and create discord. This was certainly the case, and there was well-documented bad feeling between Bennett and Cochrane, head of 5 Group (who, later on, performed the Dams raid).

The problem for Lovell was that someone had informed Harris, the head of Bomber Command, that:

'the responses as seen on the PPI did not correspond to objects and areas which one would expect to see and that if an observer homed onto the most prominent return on his tube it would in any cases turn out to be other than a worthwhile objective.'
Although we were ourselves most dissatisfied with the appearance of the PPI picture these criticisms were a nonsense.⁸⁰

The display *was* very difficult to interpret, and relied on being operated by people who were very skilled in making these interpretations. However, in this case it was quite possible that internecine jealousies were not aiding judgement, and that someone was rubbishing H₂S to Harris in order to get at Bennett. The fallout from this was that Lovell had to arrange further *demonstrations* at a period when he needed to be conducting

⁷⁹ Lovell (1991), p143.

⁸⁰ Lovell (1991), p141.

more *trials*. Fortunately, a plea for help to the Bomber Development Unit provided him with several staff to assist O'Kane. After the crash he was the sole person left alive who had any experience of flying with H₂S. As such he had a very heavy responsibility and workload, and it was imperative that he passed on his knowledge as quickly as possible.

The next step was to send an H₂S-equipped aircraft to BDU for trials. Lovell had to delay these from the end of August to the end of September because of further problems with the feed arrangements, when Halifax W7808 with the first EMI-produced installation finally went to BDU. This proved extremely fortunate for Lovell, though, as the EMI units were not prone to as many of the problems that beset the TRE-built equipment, as I shall relate.

Another problem for the group at this time (early Autumn 1942) was opposition to H₂S from America. Two British personnel, Bowen and Robinson, were working in America at this time (see chapters 2 and 4 for their earlier contributions at TRE/AMRE). The Americans were developing their own form of centimetre ASV and began to turn their attention to whether the British experiments with centimetre radar were worth learning about. The Americans interpreted their town-finding results much less favourably than in Britain, and in a visit to TRE in July 1942, Rabi (the head of the US Radiation Laboratory) 'advocat[ed] violently the abandonment of H₂S'.⁸¹ According to Lovell, Dee received a letter from Robinson which purported that H₂S didn't work in America, where they failed to find any distinction between the ground and built-up areas. There is a note to that effect in Dee's diary, dated 21/9/42:

Robinson reported town not seen on H₂S in US at <10 miles even at 4000 feet!⁸²

Lovell's interpretation of this was that Robinson was simply 'expressing the reasonable doubts of a scientist on the basis of tests carried out in an atmosphere far-removed from the political and military pressures to which H₂S was subject in the UK'⁸³.

⁸¹ Dee (unpubl.), 5/7/42.

⁸² Dee (unpubl.), 21/9/42.

⁸³ Lovell (1991), p146. In a letter from Robinson to Lovell (1/11/90), Robinson says that he believed that the U.S. researchers were not opposed to H₂S, but simply expressing doubts about it.

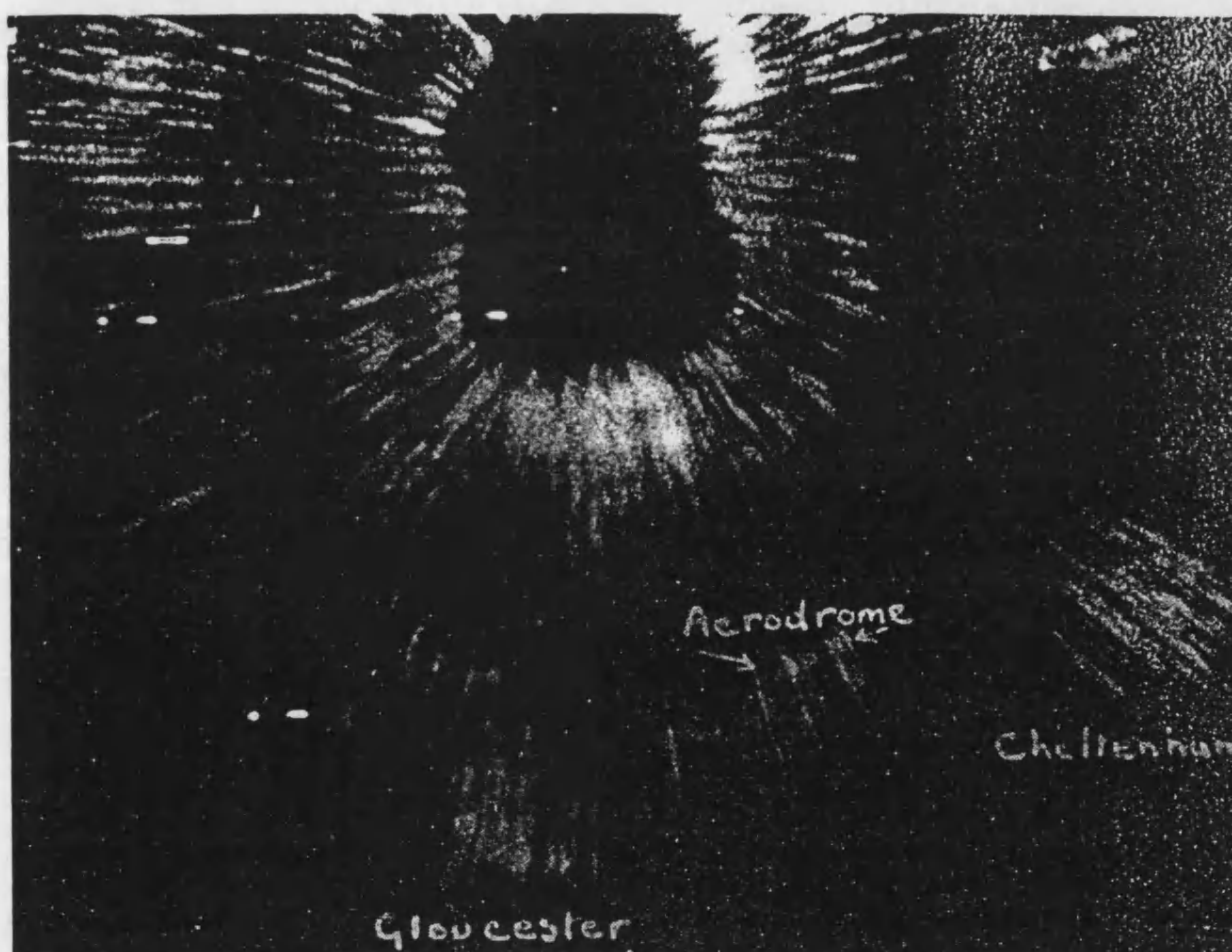


Figure 5.6: Poor H₂S picture quality, September 1942. The central ring shows the ground return quite clearly, which made interpretation of close-range images very difficult. From Lovell (1991), p145.

The work is going well. BDU are pleased and the gaps are quite gone from the Halifax... we spent the day at BDU talking to the navigators using it.⁸⁶

Here Lovell's quote illustrates the co-operation between the BDU personnel conducting the trials, and the TRE staff making the equipment. The BDU navigators were evaluating the usability of the equipment in the hands of people who were skilled in the sense that they could learn to interpret the pictures, but not in the sense that they could build it. The extra month of experimentation had made the difference, coupled with the more reliable engineered EMI units.

The above quote is perhaps the crux of the issue of the initial success of H₂S. The important thing for TRE was for them to persuade the RAF that they were giving them something that would be of assistance to them, and that would enable them to have more means at their disposal to navigate with than they presently had. So, although H₂S failed to live up to expectations in one sense, in another it was better than what they had had before. Therefore this is probably why, when EMI had made some improvements to the picture quality, objections began to melt away.

O'Kane, an expert at flying with and interpreting H₂S pictures, taught the new operators his skills. By doing this he was able to make H₂S *usable*. Lovell commented in 1991 that 'the skilled navigators of BDU found even the poor quality PPI picture invaluable when flying above thick cloud.'⁸⁷ This was definitely an improvement on September, when Dee noted in his diary that Saundby (Deputy to Harris) had commented to the effect that H₂S was "too complicated for all but an expert to use at this time."⁸⁸ However, they had improved the quality of the picture to the point where BDU's main complaint about H₂S was not its usability, but its serviceability in the rushed form with which they had been supplied. In November, after two months of experience with flying with it, BDU recommended that it be used as a navigation aid throughout the whole journey, and not just for blind-bombing. This meant that one of the original

⁸⁶ Lovell (1991), p148.

⁸⁷ Lovell (1991), p148.

⁸⁸ Dee (unpubl.) 19/9/43.

specifications, scrapped in July, had been reinstated after doing further experiments and trials.

Over the next two months EMI rushed to build 50 sets and equip two squadrons with the device. The final go-ahead about whether they could be used depended on Churchill's interpretation of the war situation. During December 1942, the Russians turned the tide at Stalingrad, and although the war situation was still far from hopeful, Churchill authorised use of magnetron-equipped aircraft over enemy territory. H₂S was first used operationally on 29th/30th January. Three nights later, on the second time it was used operationally, a set was lost over Holland. This set was found by the Germans.

That H₂S was made useable in such a short space of time, was because the navigators were now able to learn how to interpret the image to navigate successfully. The problem, according to one navigator, was to:

...interpret correctly the seemingly indiscriminate arrangements of tiny glows that flickered in curious patterns with each rotation of the scanner. Somehow I had to learn what represented salient features on the ground and ignore the mass of ill-defined reflections.⁸⁹

The navigators had to develop the same embedded skill of learning to see, that the scientists did. Fortunately for them, the scientists had established what the correct thing to "see" actually was. The navigators could be taught by learning from the embodied knowledge, that the scientists made explicit for them.

When trials began at BDU, one of the navigators who worked with it was Flt. Lt. Killip. It was his report that led to the November recommendation. He was partly responsible for some of the changes that were made in map representation and navigation technique that came about through its introduction. He outlined in a 1945 paper the impact that H₂S had when it was introduced:

The original conception was a device to enable the operator to bomb a built-up area when placed within 15 mile (24km) by some other means. It was quickly appreciated, however, that here was a device with which, using care and imagination,

⁸⁹ Mayhill (1991), p27.

a navigator could find his way around Europe independently of ground organisations and limitations of range.⁹⁰

Like the other navigator (who was actually a bomb-aimer, of which more shortly), Killip explained that there were difficulties with navigation:

It was soon realised that straightforward map-reading by H₂S was quite out of the question. The picture was not easy to interpret, particularly in industrial areas, and it was decided that fixes by bearing and distance should only be obtained from really large towns or well isolated smaller towns.⁹¹

In order to assist the navigator, air-maps were redrawn to show salient cities in the shape that they appeared on the screen. This was done on the recommendation of Killip after extensive air trials. The Operational Research section started to assemble PPI Pictures in order to assist in map-making of targets in November 1943. ORS also set up an H₂S training programme.⁹² This marked a change in convention between air maps being representations adapted from road maps, to air maps being representations designed for use with a piece of airborne equipment.

The system was now used in conjunction with dead reckoning navigation. The navigator would plot a track on the map, and any town that gave a suitably distinct image on the screen and was within 15 miles of the aircraft's track should appear after a given time depending on the relative ground speed of the aircraft. The navigator could then use the appearance of towns to check that his wind-speed information was correct. If the town didn't appear, or was not on the correct side of the track, then the aircraft was off course. However:

The difficulty... was this; would the navigator be able to cope with both the DR navigation and the H₂S interpretation? Apart from the exceptionally capable few, this seemed impossible, and the bomb-aimer (in P[ath] F[inder] F[orce]) often a qualified

⁹⁰ Killip (1985), p399.

⁹¹ Killip (1985), p399.

⁹² AP3368. Unfortunately, I have no examples of what the changes actually looked like.

navigator) was brought in to make a navigational team of two. These two sat side by side, to ensure good co-operation. The navigator would work on [dead-reckoning] and notify the operator when it became necessary to search for the next land-mark. The latter would meanwhile be watching the PPI, with a map in his hand, endeavouring to identify the landmarks independently. If at any time a particularly definite landmark was noticed, he would inform the navigator and make a fix.⁹³

It was through the experience of actually working with the system that these changes were introduced. Using the system required considerable interpretative skill on the part of the navigator. This was made slightly easier with the redrawn maps, but some navigators were never able to use it effectively:

Some of the navigators and bomb-aimers thought H₂S a dead loss and not worth the trouble, but I wanted to do more in the air than map-read, drop the bombs and man the front guns, ...⁹⁴

Utilising it effectively depended very often on what lay on the track, as some features, especially water-based ones such as distinctive rivers or oddly shaped towns were represented much more clearly (see overleaf for picture):

The bomb-aimers... pencilled in the tracks and ETAs and sorted out prominent water and urban features which could possibly be deciphered on the H₂S screens.⁹⁵

What was important was that a considerable degree of interpretative skill was needed by the operator for him to make sense of what he saw, and that skill was something that had initially to be passed on by the persons who developed the original apparatus and first learned how to do the interpretation.

⁹³ Killip (1985), p400.

⁹⁴ Mayhill (1991), p27.

⁹⁵ Mayhill (1991), p51.

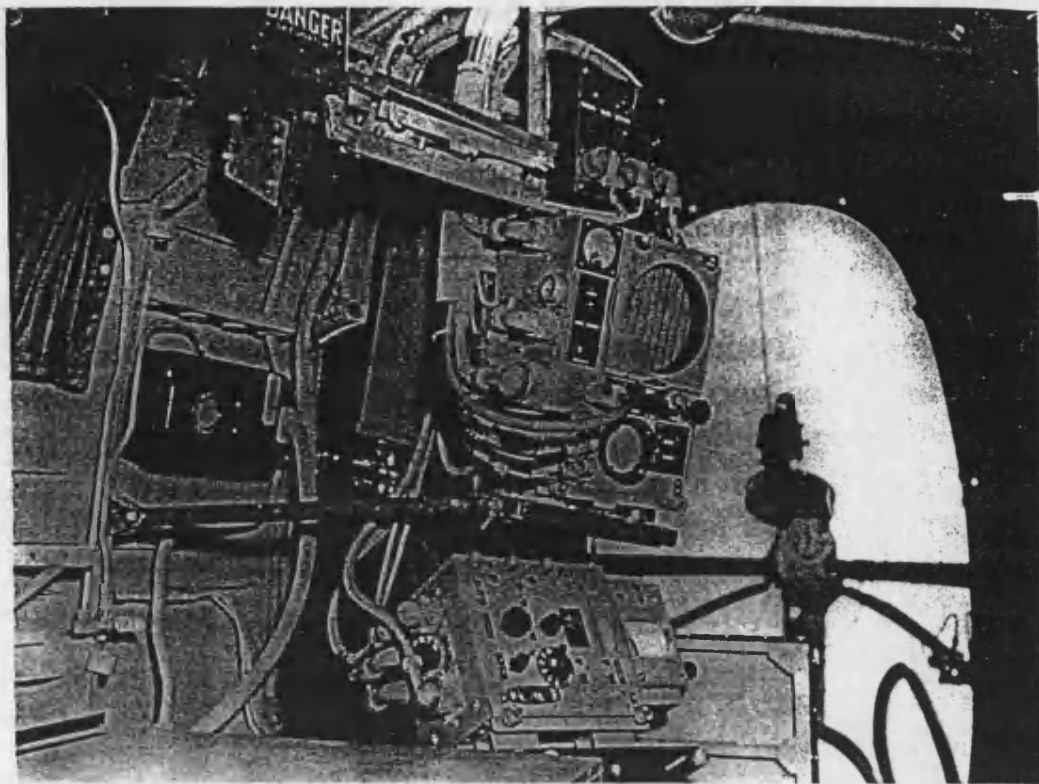


Figure 5.7: H₂S indicator unit in a Halifax. From Lovell (1991), p145.

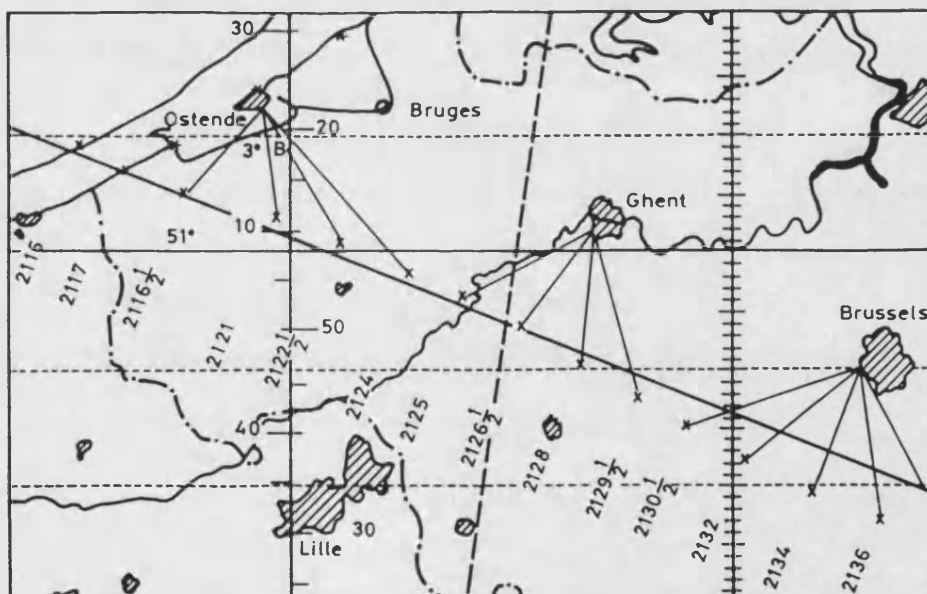


Figure 5.8 Navigation with H₂S. The navigator / bomb-aimer would take fixes by using timed intervals to easily recognisable large towns. This became easier with the introduction of special maps that represented towns and cities in the same way as they would appear on the H₂S screen, as this map does. From Killip (1985), p400.

5.7 Conclusions

By the time H₂S was first used operationally, it had become a relatively stable artefact in terms of its design and construction. All the main decisions about what the purpose of the device was, and how it should be constructed had been taken. To a large extent, the uncertainties concerning the design of a scanner and feed arrangement that would produce a meaningful PPI picture were also resolved.

What can be drawn from this is that at this stage, the skill of the experimenters in making 10 centimetre radar work had been embodied into the equipment. However, in terms of the picture, there was another area of skill to be addressed. This was the skill of learning to interpret the picture.

As can be seen from the illustration 5.6, what was produced on the screen of an H₂S radar was nothing like a map, which was the representation that most navigators would be familiar with. The scientists who were familiar with the programme of trials with the equipment were used to making allowances (reparation) for what they saw. They had developed skill in interpreting the 'splodges' as something meaningful that they could interpret. In the period between October and January they were able to teach service navigators those skills, so that H₂S could be used effectively on missions. This particular skill transfer also forced a change in the way that towns were represented on maps.

Chapter 6: Radar Development in Germany Prior to the Discovery of H₂S

6.1 Introduction

In the previous three chapters I have covered the complete story of British radar, both metre wave and centimetre wave, since its inception in 1934/5 to the point of the development of the H₂S ground-mapping centimetric radar in 1942/3. Over these eight years, four of which were during war-time, British researchers made considerable advances in the field of radar development, and particularly in the field of centimetre-wave generation. Until 1940 they had little input from outside the country. In this year some information came into their hands from France (see chapter 2), and some came from the US after the Tizard Mission (see chapter 4). In particular Britain began a programme of co-operation with the US particularly in the field of centimetre waves, but it took until late 1942 before this really came into effect. Radar development in each country was until this time largely independent. The first centimetre radar to be used on flights over Germany was the H₂S system. It was installed into British Bomber aircraft during December 1942/January 1943, and was used operationally for the first time at the end of the latter month. The second time it was used, on the night of 2/3 February, one of the H₂S-equipped aircraft was shot down over Rotterdam in Holland, and pieces of the equipment were recovered by German investigators. Once the Germans realised what they had been presented with, they too commenced a centimetre research programme.

In this chapter I shall cover the progress made in Germany in designing and developing radar systems prior to the discovery of the British H₂S set in February 1943. This date was a watershed for German radar development. The discovery led to a complete change in both the direction and the organisation of how research and development of radar was conducted in that country.

German workers began research into the possibilities of using radio waves for location in 1933, at around the same time as scientists in the other countries interested in this area. German research structure differed from British, in that the different armed services contracted out work to private firms to the exclusion of doing work in Government

establishments. There was very little co-operation between the firms and the services, and little research work was done in Government Institutions. Competition and secrecy between firms and the Armed Services was a major feature of German research and development during the war.

Their researchers commenced experiments on very short wavelengths, of the order of centimetres, but were hampered by the lack of an electronic valve to generate these short waves at a suitable power. Because of this, they moved to longer wavelengths in order to get the power they required. This was also very different to the British, who commenced their research on metre-wavelengths and moved to shorter ones when the technology became available.

The German strategic outlook differed greatly to that in Britain, being primarily offensive in the first half of the war, and defensive in the latter. The strategic aims of the Germans were based around Hitler's plans for *Lebensraum*, or conquest of Eastern Europe for "living-space". Despite Hitler being an arch-opportunist in the way he set about conquering the territories he wanted, German military strategy was based around the aggressive waging of war and radar, thought of as being a defensive aid, played little part in their thinking. Their war-aims dictated the way they employed their radar technology, much the same as it did in Britain where Air-Defence was the prime-mover behind radar development. In Germany, the offensive stance led to the development of some superior radio bombing aids, but these were based on the Lorentz blind-landing system and so cannot be classified as radar. This stance meant that often where radar equipment of a high technical standard existed, in hindsight one could judge that it had not been used as effectively as it could have been. For example, the preparation of an air-defence strategy was completely overlooked by the Luftwaffe until events forced a re-think, in 1941. Early on in the war, between 1939 and 1941, British air-raids over Germany were ineffectual (see chapter 5). Consequently, air-defence was afforded a much lower priority than air-offence, and little thought was given to how radar might best serve this application. It was only in 1942-3, when British bombing became far more than a minor irritant (helped by radar aids) that Germany was forced to develop an early-warning and centrally-controlled air-defence network comparable to that which existed in Britain in 1940.

6.2 Developments in the 1930s

6.2.1 The First Interest in Radio Reflection and Detection

– The first ever detection apparatus to use radiowaves originated in Germany in 1904, in the shape of the telemobiloscope of Christian Hülsmeyer (see chapter 2). The origins of the first attempts to produce in Germany a *practical* means of detection using radio waves can be traced back to the early 1930's. The origins of radar in Germany were to come from the direction of centimetre waves. Future events were to prove this deeply ironic.

The experiments performed in many countries concerning ionospheric work, and the developments made in component design in the 1920s and 1930s were widely published. One can assume that the results of this work from other countries was available to Germans disposed to look for it. Germany also had a significant radio-industry of its own during this time.

In 1929, Dr Rudolf Kühnhold conducted experiments in echo-location for the German Navy. He was the Scientific Liaison Officer with the Civil Service Department of the Navy Communications research establishment at Kiel. The sonic device that he developed was able to measure both the range and bearing of targets via a twin receiving system. According to Pritchard:

...it occurred to [Kühnhold] that this principle might be employed for the detection of centimetric wavelengths, and that the location of aircraft might also be possible.¹

Kühnhold was aware that there was no future with sound-locators for aircraft, because aircraft speeds were now a significant fraction of the speed of sound and were increasing. This, too, was the impetus for the beginning of research into radar in Britain (see chapter 2). This increase would in turn led to a decrease in the accuracy with which aircraft could be located.

¹ Pritchard (1989), p32.

In 1933 "a policy decision was made by the Navy to develop a radio-detection device."² This was largely at Kühnhold's instigation, and a transmitter and receiver working on a wavelength of 13.5cm were ordered from the firm of Julius Pintsch. The transmitter produced 100mw of power, and employed a Barkhausen-Kurz valve with antenna reflectors of 80cm diameter. He performed experiments where the equipment was trained on shipping in Kiel Harbour, but "only poor results were obtained. Clearly greater power was called for and this presented difficulties."³ Not much light is shed on this by any sources; Trenkle wrote only that "there were no demonstrable reflections, whatsoever."⁴

In the light of future happenings the decision to begin experimenting on very short wavelengths is an interesting one. It shows that the Germans originally thought along more adventurous lines than their British counterparts, which contrasts with later developments as I related in chapter 5 and will relate in chapter 7. At least one person in Germany was prepared to investigate the possibility of short wavelengths at the outset. What hampered him, as indeed it hampered the later British, was the lack of a suitable high-power valve in order to carry out research. The state of centimetric valve research at this time in Britain and America is covered more fully in chapter 3. However, by employing near-10cm waves and parabolic reflectors Kühnhold's thinking was, in the context of the successful direction taken in 1939/40 in Britain, very advanced. This should be borne in mind when considering the later German dismissal of very short wavelengths.

6.2.2 Commercial Concerns

Apart from Kühnhold's personal interest in the reflective properties of electromagnetic waves, the other interested party was his financial backers, the Torpedo Research Establishment of the German Navy. Their interest was in gaining greater accuracy in determining the range of their weapons.⁵ This is in contrast to the British, who were

² Swords (1986), p92.

³ Pritchard (1989), p32.

⁴ Trenkle (1978), p25. Translated from the original German.

⁵ Pritchard (1989), p31.

originally motivated by the desire to detect aircraft. In other words, the first German service to take an interest in using radiowaves as a means of detection did so because they saw them as a research tool, whereas in Britain the technological air-defence application was the driving force behind the research. It is possible that it was the German's initial lack of success at short wavelengths that shaped their future attitudes to their exploitation. Very short waves were mistrusted after longer wavelengths satisfied the performance criteria of the Armed Services. This effect was certainly true in relation to attitudes at TRE to the klystron, which was regarded as a non-starter for H₂S in 1942, because of the difficult nature of the klystrons employed in AI research during 1940.

Kühnhold's initial lack of success led him to modify his equipment by purchasing a 48cm wavelength glass-envelope, split-anode magnetron from Phillips in the Netherlands. These were a longer wavelength and a higher power than the previous valves he had used. Disappointed with the results, he also reverted to Yagi antennas in preference to the paraboloids. In any experimental set-up there are a large number of variables which could be contributing to problems when, as is usually the case, results do not match expectations. It is more likely that, rather than change every part of his set-up in one go, the experimenter will modify it in stages to lessen the impact of new and unknown components. This approach was taken by Faraday in his experiments,⁶ and it was also used by Morpurgo when developing his quark apparatus.⁷ Changing components is also often a source of difficulty in replication.⁸ These effects also occurred in British experience, as I relate and discuss in chapters 2 and 4. The paraboloid antenna was less well understood or used than the Yagi antenna at this time. Kühnhold was trying to improve his result with a better-understood and more familiar component. By doing this, he was limiting the possible unpredictable components of his apparatus in order to proceed with the learning process more easily. This fits with Gooding's analyses of other scientists' learning endeavours, which illustrate that it is by practical interaction with their equipment that scientists learn *how* to operate it.

⁶ Gooding (1990), ch 6.

⁷ Gooding (1992).

⁸ Collins (1985), ch 3.

As well as not obtaining satisfactory results initially, Kühnhold also had problems with funding. The Torpedo Research Establishment had only a limited research budget, and were not overly enthusiastic about funding the further experiments that he planned to do. He tried to get further backing by making approaches to other German companies working in radio research. No-one else was interested in taking up the work which was regarded as risky and not very promising, so he decided to set up his own company along with his deputy, Dr Schultes, called GEMA (Gesellschaft für Elektro-akustische und Mechanische Apparate; Company for Electro-acoustic and Mechanical Apparatus).

In late 1933 the Pintsch Company began experimenting with 13.5cm valves, using continuous-wave rather than pulse operation, just as Kühnhold's apparatus did. Their experiments led to them to conclude that very short waves performed better over land than at sea, where reflections from the sea obscured the signal. They managed to achieve a range of 2km on a ship target in May 1934, but the results they got over land were better, doubling this figure.⁹

Learning of these results, and being spurred on by the competition under which German firms hoping to supply the military worked, Kühnhold's team under Schultes set about improving their apparatus. One of the problems they had encountered with the CW operation of their equipment was that of stray radiation from the transmitter leaking into the receiver. This was a perennial problem for the early radar designers, which produced a number of different solutions. In particular this problem dogged the British AI until 1941 (see chapter 4). Kühnhold's new apparatus, using 1.5m paraboloids with Yagi antennae and a wavelength of 48cm, enabled him to produce far better results. The competition with Pintsch led to comparative testing between the two sets, during which they discovered that target aspect affected the received signal-strength, particularly so in the case of the 13.5cm Pintsch equipment. This result is important, as it is very likely to have had an effect on the perception of the reflective properties of 10 centimetre waves in German radar circles. This probably contributed to the anti-10 centimetre attitude, which led to the cessation of experiments in this area in 1940. Such antipathies also existed in Britain, in the form of opposition to the klystron for H₂S, and were a function of practical experience of actually

⁹ Pritchard (1989), p39.

working with the apparatus to perform experiments.¹⁰ This contrasted to German objections which, as I explain in chapter 7, were based on theoretical calculations of how centimetre waves would behave.

By October 1934 GEMA felt sufficiently confident with their equipment to conduct a demonstration. There is an important distinction between a demonstration and a test; testing is usually done by experimenters when the apparatus is not properly understood and cannot be made to work properly *to order*. Demonstrations usually involve the public display of a technique or effect to convince others that such an effect is a “fact”. The experimenter(s) are sufficiently competent enough at working with their apparatus to be able to produce the effect, or to know where to look and what to do if it doesn’t happen. When the distinction is blurred things are usually difficult for the experimenters, as was the case with Lovell and the pressure he faced to demonstrate H₂S when it was extremely new and un-tested. He was unable to demonstrate how it worked because he had not completed tests to learn *how* it worked.

This demonstration took place on October 24th 1934. Their equipment, working on a wavelength of 48cm, used two paraboloids: one for transmitting and one for receiving. The indicator was a voltmeter (unlike the British who used CRTs), and the apparatus operated in the CW mode. The transmitter and receiver were separated by 200m to try and reduce waves leaking from one to the other and affecting the received signal. The results of the demonstration were successful, as they managed to detect the research ship Welle, and in addition 'saw' an aircraft passing overhead at a height of approximately 200m to a distance of 700m.¹¹

An official of the Torpedo research Establishment was present at the demonstration, and recommended that 70,000 Marks be allocated to GEMA for further development of this equipment. The money was also accompanied by a set of recommendations; chief among these were that they should move to pulse operation, which would have several advantages. These advantages were those recognised in Britain, namely that a transmitter valve could be pulse to give a power output significantly higher than that gained during CW operation, as

¹⁰ Swords (1986), p94.

¹¹ Swords (1986), p94.

the valve was switched on and off very quickly which would not damage it as much (damage could still occur as Burcham and Atkinson often found out when trying to get ever higher peak-pulse powers from their klystron in 1940). Also, pulse operation would assist them to find a solution to the problem of leakage between the transmitter and the receiver by allowing them to isolate the two through blocking off the receiver when the transmitter pulse was in operation. Thirdly, range measurement would now be a possibility for them, especially if they made this improvement in conjunction with using a cathode-ray oscilloscope (This was also the method adopted in the British demonstrations). Other recommendations concerned specific types of valve and antenna that they considered to be improvements over those used in the demonstration.¹²

Unfortunately for the historian, there is very little *detail* available from any source about these early experiments. The greater number of sources which I have utilised for German material were secondary, as is evident from the footnoted date of the source. However, most of the secondary sources have a considerable primary input, as the authors either had access to original documents or were able to interview participants. Nevertheless, this should be borne in mind when one compares this information to the richer nature of primary British sources I have used in chapters 3, 4 and 5, and the German sources used in chapter 7.

From these early episodes it is possible to form an idea of the way radar research was organised and undertaken in Germany. The initial experiments were initiated by one person, Kühnhold, but utilised a team of researchers from, and the resources of, the Torpedo Research Establishment. As a Service Research Establishment was paying for the research, the directions that the researchers took and the applications they considered were very highly geared towards what that Establishment wanted. Competition between firms for contracts, and the ensuing secrecy was a feature of German radar research from very early on. Such competition and secrecy between supposedly co-operating firms and services became a distinctive feature of the German radar research-effort, and these two factors differed greatly from Britain in the sense that they were officially encouraged. In Britain, by 1939 at least, there was supposedly official co-operation between firms, Services and Establishments

¹² Swords (1986), p95. This is taken from an unpublished manuscript by H.K.V. Willisen, c.1950, titled *Die Geschichte der Funkmesstechnik bis 1935* (The history of radar-technology until 1935), and given to Swords by F. Trenkle.

through CVD (see chapter 3). Other features that distinguished German and British research, such as the lack of one distinct research institute in Germany, will be discussed later.

In technical terms the approach of the German engineers differed quite markedly to their British counterparts, which could help to explain some attitudes that developed in Germany towards centimetre wave radar in the near future. The Germans set about the problem of detection by utilising the shortest waves they could generate, even at the very low powers at which this was then possible. They also began their experiments by adopting the continuous-wave method, rather than going for pulse operation. When results were hard to get, they changed their apparatus to use longer wavelengths at which higher powers were possible. This was in contrast to the British, who started with a much longer wavelength because they felt they understood the technology more fully. Moving down the wavelengths was only seen as useful by *some* British researchers, and then only in conjunction with the specific AI project.

The experiments conducted at GEMA and Pintsch on 13.5cm did not give very convincing results. As a consequence of this, and the inability to improve the power available at these short wavelengths, both companies concentrated on higher wavelengths. However, one important discovery which they made was about the reflective behaviour of waves in the 10cm region. They noticed that the waves were scattered differently according to the orientation of the target (aspect), a result that a few years later was influential in the decision to stop research on radar in this wavelength area.

6.3 Developments to the Outbreak of War

The story of the development of German radar from 1936 up to and during the early part of the war is very similar in style to that of their initial developments in 1934/5. One of the more significant things that set German radar development apart from the British equivalent was that most development work was undertaken by the commercial sector as opposed to in Service or Government research establishments, as I will relate. Britain was

the exception rather than the rule in having Government sponsored radar research towards a perceived goal. At this time most other countries were following a similar pattern to Germany.¹³

In this section I shall cover the progress made in Germany up until the beginning of the war. Because of the way German research was organised, I shall describe the events by looking at the research done by each the major firms involved in radar research in separate sub-sections, rather than by tracing development of particular types of radar (as I did for the British in chapter 2).

6.3.1 The Telefunken Company

During the 1920s and 1930s Telefunken was the largest manufacturer of electronic equipment in Germany. It also had a significant research and development department based in Berlin. In 1924 Dr Wilhelm Runge took over as head of Telefunken's radio receiver laboratory. Over the next few years he worked hard to expand the laboratory in terms of the number and quality of its staff and the nature and scope of the research work they undertook.

In early 1934 Kühnhold approached Runge and requested that Telefunken take over his embryonic radar research programme. At this time Kühnhold was having difficulties in getting enough finance to continue with all the work he wished to do. Kühnhold was aware that Runge was already engaged in work on short radio waves, with regards to using them for directional radio transmitters, and reasoned that he would be the best person to assist him in carrying on the work.

Unfortunately Runge didn't exactly welcome Kühnhold with open arms. His attitude was not positive, and he sent Kühnhold away empty handed. According to one author:

¹³ For further discussion of the way different strategic goals can affect the way the research process is organised and executed, see Beyerchen (1994).

Als Dr Runge sich damals etwas skeptisch äusserte, fasste dies Dr Kühnhold als offizielle stellungnahme der Firma Telefunken auf und veranlasste im Jahr 1934 die Gründung einer neuen Firma, GEMA.

[As Dr Runge at that time appeared somewhat sceptical, Dr Kühnhold took this as being the official position of Telefunken and this gave rise him founding a new firm, GEMA, in 1934.]¹⁴

Several authors relate that Runge and Kühnhold didn't have good relations.¹⁵ Whilst disagreement between researchers on approach, or viability of certain ideas was not uncommon on either side, the nature of the way German research was organised exacerbated this disagreement. By encouraging secrecy and competition between services and between firms the government produced a fertile environment for personal animosity. In Britain the culture was more inclined toward co-operation, although this did not always prevent personal disagreements, as I have shown in Chapters 2 and 4 when I outlined how the animosity between Rowe and Bowen probably delayed both AI and H₂S.

When research into the possibilities of radio waves for detection purposes began in the early 1930's, Germany was in the grips of the World depression. Understandably the majority of firms were unwilling or unable to spend money on the sort of long term research and development projects that radar represented, unless they also were in reception of assistance in the form of Government or military funding. Hitler's accession to power in 1933, and the accompanying expansion in terms of military spending, went some way towards ameliorating this situation, but it left a legacy of caution on the part of most commercial concerns towards undertaking risky ventures. This partly explains Runge's sceptical attitude towards Kühnhold's proposal.

However, Hitler's accession also had other effects, one of which was a change of personnel in many firms. These changes were due mainly to a person's Nazi Party links or racial origin; for example many Jews left voluntarily, or were forced out of what were often key positions in firms. This policy had a marked effect at Telefunken where Runge worked.

¹⁴ Trenkle (1986), p24.

¹⁵ See Swords (1986), Pritchard (1989), Kern (1994).

Apart from Runge's interest in short radio-waves, which led him to undertake a small amount of research involving American "Acorn" valves (see also Chapters 2 and 3), there was a further group at Telefunken interested in centimetre waves. This group was led by a Dr Ilberg, and they co-operated with a another group at the Telefunken Physics Laboratory under Dr Meissner. They carried out research into the characteristics of Barkhausen-Kurtz valves, the most often-used source of centimetre waves (albeit at very low powers) at this time.

According to Runge, in 1934:

We had a man in the firm called Captain Scharlau, who was advisor to our Export Sales Group. It turned out that he was an old party member... and one of the first jobs he was ordered to do... was to clean the firm of what [the Nazis] called the "opposing elements", in other words the Jews. Our director, Emil Mayer, was the first to go, and with him of course most of the company top brass. Mayer was replaced by a man called Schwab... and he, too, was... replaced by Dr Rukop.

Scharlau demanded that the company leadership, which to then had always catered for customer requirements, should now be reorientated. Telefunken must now look forward and carry out more research. They gave me an interim grant of 20000 Marks for early development (such as it was) of centimetric systems, and ordered Dr Ilberg's group to come under my jurisdiction. We made the best of a bad bargain...¹⁶

Runge's centimetre group worked initially on investigating the directional qualities of very short waves in order to use them in a directional radio-telephony link for the army. In the course of their work they experimented with using Frequency Modulation (FM) in order to get a clearer signal (this method cuts out much of the noise from an analogue signal). During tests of their system at Friederichshaven in 1935, in the manner of several researchers over the years in different countries (see chapter 2), they noticed interference with their signal from passing ships and aircraft. In the summer of that year Runge decided to investigate these reflections further. He put a transmitting antenna on the ground, connected to a continuous wave transmitter, and placed a receiver next to it. As he described:

¹⁶ Pritchard (1989), p58. Quotation taken from Runge's memoirs (unpubl.), given to Pritchard by Runge.

As soon as I had switched everything on, I happened to notice one of Telefunken's own Junkers Ju52s at 5000 metres... and as it drew closer to the directional beam I noticed the [indicator] begin to flutter... [A]s the aircraft flew overhead, the needle went right over and began to move slowly between zero and full-scale deflection.¹⁷

The behaviour of the indicator was indicating a “beats” effect similar to that employed as a means of detection in the Daventry Experiment (see chapter 2), except that here it was a variometer that was being used as a detector rather than a CRT. In a curious reversal of Daventry, where the received signal was being calibrated by the appearance or non-appearance of the signal in relation to the aircraft's passing, here Runge already accepted that it was the aircraft that was causing his indicator to move so the calibration of the “out-thereness” of the aeroplane was already confirmed in his mind.

When Runge took these observations to Rukop, his department head, the latter treated them with the same scepticism that he had shown to Kühnhold. Undeterred by this official apathy, Runge pressed ahead with his own experiments, apparently unaware of the work going on at other firms. He set a colleague, Dr Muth, the task of designing a pulse-circuit that operated at the tiny power of 15 watts peak. The pair continued their unofficial experiments until 1938, gradually learning more about how to improve their apparatus until they had a much more impressive system employing a 3m parabolic antenna. By this time they must have been pretty certain of how to make it work, as they now felt they could arrange a demonstration (rather than a test) to show they could detect aircraft at a range of 5km. They showed their system and demonstrated these results to the Head of the Luftwaffe Procurements Section, General Udet, in the summer of 1938.¹⁸

In the meantime Kühnhold's GEMA had not been inactive, and in the intervening years they had developed a fully-operational early warning system called *Freya* which they had sold to the Navy. The Luftwaffe, in the manner of the times, was jealous of the Navy's

¹⁷ Pritchard (1989), p62.

¹⁸ Pritchard (1989), p64.

equipment, but didn't want to purchase exactly the same model as the Navy had. Consequently, they ordered their own system from Telefunken. As Runge described:

To give you some idea of the state of chaos prevailing in the Services, the Luftwaffe in particular never seemed to know what they wanted. One department said one thing, and this was usually countermanded by another, but by 1939 things were beginning to sort themselves out, and in that year we received a firm order for an early-warning system which had to be much smaller than [GEMA's] *Freya*, and, more importantly, extremely mobile. To this end we designed an equipment with a 3m diameter parabolic reflector and a fixed dipole... Someone stuck a pin in the map and we called it the *Würzburg A*.¹⁹

In this respect the Germans were hampered compared to their British counterparts, who had a very clearly defined set of operational parameters to work with. They were working with different perceptual filters depending on the strategic aims they were trying to meet; these determined the technological issues that were addressed.

Perceptual filters to technological development were very important to the story of German radar development, and the issue of the difference of technical, technological and operational change are explored by Beyerchen.²⁰ He identifies the major differences thus: technical change is a matter of specific equipment, e.g. upgrading. Operational change is a change in the way specific equipments are employed. Technological change is a change in the context of the way wars (for example) are fought because of the introduction of a new technology, such as radar. New technologies shape the strategies of the possessors, and initially both the British and the Germans viewed radar as essentially a defensive technology. This suited British strategic thinking which in the late 1930's was almost exclusively given over to defence. However, in Germany the lack of any well-defined perceptual goal meant that very often firms were not sure what they were supposed to produce. If they did know, they often produced only what they *could* produce given their current knowledge, rather than pushing in new directions to satisfy a far-reaching military proposal (as was the case in Britain with centimetre AI).

¹⁹ Pritchard (1989), p65. Quote from Runge's memoirs, unpubl.

²⁰ Beyerchen (1994), pp169-283.

However, Runge's somewhat dismissive statement quoted above plays down the extent to which the *Würzburg* was an extremely well engineered piece of equipment. It was this set which was later captured in a commando raid on the French coast in early 1942 (which precipitated the move of TRE from Swanage to Malvern, see chapter 5), and which brought nothing but praise from the British experts who inspected it. Their praise was directed towards the quality of the engineering of the set, as:

- (i) it was a robust, well-designed equipment,
- (ii) it could be operated by relatively unskilled personnel,
- (iii) it had a number of "black-boxed" units designed for easy removal and replacement in the field,
- (iv) the set operated at a wavelength of around 48cm, and at powers of 8kw.

That this was possible was due to an incident in 1938, when the head of Telefunken's R&D department, Rukop, accompanied Kühnhold of GEMA on a visit to the Navy. Kühnhold was critical of Telefunken's policy of pursuing radar research at these short wavelengths, on the basis that they (Telefunken) were not up to the task of producing suitably high-powered valves. GEMA's *Freya* worked at a wavelength of 1.5m (similar to that which the British employed for their CHL system, though at this time only Bowen's experimental ASV and AI systems used such short waves).

Rukop was so incensed by these remarks that he initiated a programme to produce the requisite high-power/short-wavelength valves in a very short time. This valve was produced, and formed the basis of further experimental work that was incorporated into the *Würzburg* after it was ordered in April 1939. It entered service in 1940, and was employed in the role of early-warning.²¹

Despite there being no *details* of the sequence of experiments and modifications that were available to me, working at around 50cm was not something that was outside the bounds of current technology. Certainly in Britain, both Bowen at AMRE and GEC were using valves (the micropup) that worked at this wavelength around 1939/40. What is important is that Telefunken were the company who eventually had the task of manufacturing the centimetre radars developed from British technology. What they gained

²¹ Swords (1986), p98.

from their work at this time was an experience of working with parabolic reflectors, and shorter wavelengths. However, the really important shift in the types of technology and techniques used came in the ten centimetre region. For this researchers had to employ waveguides, matching, and special valves (conventional valve technology was reaching its limits at 50cm, see chapter 2). These were all things that were in the embryonic phase at this stage (1938/9), even if the Germans had the information from American experiments available to them. Therefore, their efforts with 50cm would have been of only limited assistance in helping them to understand centimetre equipment.

6.3.2 GEMA

GEMA was formed after Kühnhold's approach to Runge in order to get co-operation and further funding proved inconclusive. This led to their demonstration of the 48cm equipment in October 1934, and the decision of the Torpedo Research Establishment to provide further funds for Kühnhold to carry out more work.

Between then and May 1935, researchers at GEMA worked on producing a pulse transmitter with a peak power of 800 watts. Tests showed that they had made an improvement in the equipment's maximum range (it should be remembered that the operating wavelength was 48cm). Furthermore, the group started using a CRT for display, which enabled them to predict range with a much greater accuracy. The display showed both transmitted and received pulse, and had a calibrated range scale on the screen. Unfortunately, ranges attained against the ship *Welle* were only 4km, a much lower range than they had previously attained.²² In light of this they decided to change the operating frequency to 125MHz (0.8m), which would permit them to use valves with a much greater peak power, and a consequent increase in range (4km being pretty much useless for radar applications). However, this caused dispute between those who supported the change, and those who wished to continue working on 48cm.²³

²² Pritchard (1989), p39.

²³ Trenkle (1986), p27.

At this time, May 1935, British experiments were still at the rudimentary stage of post-Daventry experiment, and they were still using wavelengths of 26m. It was not for a further two years that Bowen had an operational 1.5m set. This further underlines the difference in approach to the problem of detection at this time of the British and German engineers; namely the British tended to proceed along the lines of "get it working with understood technology and then improve", whereas the German attitude was "go for what we believe to represent the best technical solution even if it means improving the technology before it works satisfactorily".

The researchers at GEMA now felt able to demonstrate their 48cm equipment to the Navy. They were not under the same pressure as Lovell was in 1942, for example, as they were doing this original work off their own bat in order to get orders for equipment. This demonstration further strengthened GEMA's links with them, as they were able to track a ship to a distance of 15km, and at an accuracy of ± 0.15 of a degree. The Navy made further research funds available on the strength of these achievements. Over the next few months they conducted further experiments, and made improvements to both the 48cm equipment and the 82cm equipment. When they found that the ranges they could obtain with the 48cm equipment were not great enough to be useful, the dispute about whether to continue with it was resolved in favour of the 82cm equipment. They reasoned that any improvements made to the shorter-wave valves would result in a double improvement in the performance of the longer wave ones. By February 1936, they had constructed an 82cm equipment and subjected it to tests. In setting it up they didn't arrange the antenna in the correct direction, which led them to detect an aircraft, rather than the ship that it was targeted at. Unfortunately there is no great detail about this fortunate "mistake", but the aircraft was at a closer range (8km) than the supposed ship-target. I would presume that they were able to calibrate the range finding, and so associated the return from the nearer aircraft. This was an important step as it persuaded them that aircraft (being faster moving and smaller than ships) were also a potential target for their equipment to detect. They performed further tests over the next few days, where they followed aircraft out to greater distances, learning to "see" with their equipment to greater ranges.²⁴

²⁴ Pritchard (1989), p41

GEMA's early research and its relationship with the Navy now presented that Service with two possibilities from the investigations they were sponsoring: detection of ships, and detection of aircraft. Each option was taken up. By 1938 GEMA had produced prototypes of two *operational* radars: *Seetakt* for ship to ship detection, and *Freya* for ship to air or ground to air detection. Both these systems employed 2.4m wavelengths, which were much longer than most other research in Germany, but still a great deal shorter than the British Chain Home, and its 13m wavelengths.²⁵

By the outbreak of the war in September 1939 the GEMA company had considerable experience in designing and manufacturing high-quality radars. Their products were well engineered, but the technology they employed was no more advanced than the British technology of the time. It still utilised conventional glass-envelope valves and conventional electrical transmission methods. What was superior to the British was the amount of time they took to get it into service; British 1.5m AI, CHL and GCI were not introduced in the form of reliable manufactured units until late 1940, 18 months after the Germans. This is because the British attitude was to carry on experimenting and modifying prototype equipment, rather than design manufacturable units from the outset.

6.3.3 Lorenz

The only other firm to become significantly involved in radar before the outbreak of war was the Lorenz Company. Prior to their taking an interest in radar development, which was in 1936, they developed a blind-landing system that entered widespread usage among the world's airlines, and was even used by the RAF. The system worked by transmitting two overlapping beams, one transmitting Morse Code dots, and the other dashes, such that when the aeroplane flew along a line in-between the two beams they added together to make a continuous tone. Straying one way or the other from the centre would give the sound of either dots or dashes in the radio operator's headset. The two transmitters would be aligned

²⁵ Trenkle (1986), p28-9.

along a runway, such that the centre of the overlapping beams would be over the approach path for that runway.²⁶

Due to the development of their blind-landing system, Lorenz had considerable experience of transmitting high-power directed radio waves. Lorenz also had an experienced valve laboratory, and in early 1936 they began experimentation into detection of objects using radio waves. Quite where the impetus for this research came from is not clear; Pritchard wrote that:

Without official invitation the valve laboratories of the Lorenz Company, under the energetic direction of Dr Herriger, developed new 'Acorn' valves as well as high power transmitting valves for their own radar research...²⁷

Lorenz began their research with centimetre waves, but extreme difficulties in this area led to them increasing the wavelength they used, where the valve technology they employed was better understood. Trenkle made the point that:

Die Engländer gingen um diese Zeit den umgekehrten Weg: hohe Empfängerempfindlichkeit und hohe Senderleistung auf Kurzwelle mit grossen, feststehenden Antennen geringer Richtwirkung; nach Entwicklung einer genauen Entfernungsmesstechnik schrittweiser Übergang zu kürzeren Wellen.

[At this time Britain went in the opposite direction: higher receiver sensitivity and higher transmitter-performance with short-waves with bigger, fixed antenna; then afterwards development of an exact range-measuring technique and gradually transferring to shorter waves.]²⁸

Possibly due to their experience of working with these systems, they were able to develop sensitive equipment that, by the Autumn of 1936, they were able to install aboard ships. Despite being viewed as "suspect" by the Nazis because of their involvement with foreign governments, General Martini, the Head of Luftwaffe Signals, took great interest in

²⁶ The OBOE system for blind-bombing employed a similar idea (see chapter 5), as did the German's own blind-bombing system *Knickebein*.

²⁷ Pritchard (1989), p41.

²⁸ Trenkle (1986), p30.

the firm and their results. As a consequence they pressed on with their developments in the hope of securing a Luftwaffe contract with its associated security and investment. Martini's association led to them being asked eventually to develop an anti-aircraft gunnery radar, which became known as the *A2-Gerät*. This system worked on 2.4m, and so employed similar technology to that used by GEMA.

6.3.4 Other Research Institutions

During 1935, in a general expansion of German Research in both the Universities and in firms, several Government Institutes came into being. This expansion took place because the Nazi Government wanted to expand the Armed Forces, which required technical and scientific research. This expansion was in contravention of the Versailles Treaty which forbade Germany a modern, large Army and Navy, and allowed them no Air Force at all. Most notable for my purposes amongst these were two institutions, the DVL²⁹ and the FFO³⁰. It is odd that although these institutes appear to have done a reasonable amount of research into the properties of centimetric waves by 1940, little of this information was utilised until 1943. Some work was done on reflectivity in 1939/40 by DVL on the reflective properties of waves of 14, 20 and 50cm. They found that the surface roughness of the reflector was more important than the wavelength used in terms of reflectivity properties of the target.³¹

This work is also mentioned elsewhere, by Röde, where it is described as having been done in co-operation between DVL and Telefunken. They used a pulsed split-anode magnetron with a wavelength of 20cm in an equipment that in tests they were able to use to follow a Ju 52 aircraft in order to get a quantitative figure for the backscattering effects of aircraft. However, according to this author, there were included some

²⁹ Drahtlos Luftelektrisch Versuchsgesellschaft Gräfeling (Wireless Air-electric Research Company, Gräfeling.

³⁰ Flugfunkforschungsinstitut (Air Radio Research Institute), Oberpfaffenhofen.

³¹ Trenkle (1986), p32.

{f}alse statements on the frequency and geometric size dependencies of the backscatter coefficient which had some influence on the development of radar in Germany later on.

Further work in the spring of 1940 used 1:10 scale models, in conjunction with 6cm radiation (Würzburg used 50cm waves). The results that the researchers gleaned from these experiments indicated to them that aircraft were extremely good at dispersing very short radio waves, to the point that the reflections would be undetectable. These results were to have a very marked influence, effectively stalling radar development at centimetric wavelengths.³²

6.4 Airborne Radar

6.4.1 Early Development

I mentioned earlier in this chapter, and in previous chapters, that the impetus for a particular application of radar came largely from wider consideration of strategic/tactical necessity. This topic is considered in detail by Beyerchen³³, where the influence of military needs on the type of radars developed in Britain, the US and Germany between the Wars is examined. I have described his views on technical, technological and operational change in a preceding section.

This issue is particularly pertinent in the case of airborne radar. As I discussed in Chapter 2, the British, in particular Tizard, were very quick to see that building an airborne radar would be a solution to the particular problem of air defence against bombers at night. Radar had come into being as the solution to the problem of *daylight* air-defence. Night-time air-defence was still viewed as a significant difficulty.³⁴ Tizard's perceived solution to

³² Röde (1988).

³³ Beyerchen (1994).

³⁴ It is interesting that the British perceived that airborne radar in itself, in conjunction with Chain Home, would be sufficient to allow successful night-time air-defence. However, in practice, as is very often the case, airborne radar proved nearly useless without a lot of additional technical back-up in the shape of GCI ground-control radar, which was more accurate than CH, improved aircraft designs fast-enough to catch the German bombers, and the practical skills that had to be evolved by the pilots and radar-operators in learning

this problem was the installation of a miniature radar set into fighter aircraft, and Bowen was set the task of developing such a set in 1936. In a reversal of their policy with ground-based early-warning radar, where known technology was employed, Bowen and Tizard knew that the current technology was far from up to the job they wished to do.

However, in Germany an altogether different attitude and situation permeated almost every level and branch of radar design, development and operation. These views were coupled with a completely different set of assumptions of how any future military operations would be conducted, and consequently what part (if any) radar would play in strategic thinking. This affected the development of their radar in the following ways:

- (i) German strategy was offensive, and used the Blitzkrieg³⁵ tactics of quick, motorised assaults on a narrow front to penetrate enemy lines. Aircraft were attached to armoured groups and used for close-air support of the military. As a consequence air-defence, and the use of radar to aid it, was not a military priority.
- (ii) The lack of interest in air-defence did not produce the same urgent requirement for the development of airborne radar, as it did in Britain.
- (iii) German firms and research interests were not working together on this problem, as was the case (at least eventually), in Britain.

Conversely, scientists in Britain had started experimentation into airborne radar in 1936, and by the outbreak of war had considerable experience in the problems associated with such sets (see chapter 2). The Germans *did* have some experience of putting advanced radio equipment in aircraft, but this was through their X-Gerät apparatus. This was a radio bombing-aid developed from the Lorentz landing system mentioned earlier.

to use their equipment to make successful interceptions. All this meant that although airborne radar was first operational in 1939, it only really became useful by late 1940/early 1941.

³⁵ *Blitzkrieg* (Lightning-war) was a revolutionary method of conducting warfare that utilised swift-moving armour attacks on a short front to punch holes through defences quickly, and encircle the opposing forces. It was also, of course, an essentially aggressive strategy. *Blitzkrieg* also relied heavily on closely integrated air bombardment in support of the ground forces. Therefore the *Luftwaffe* was organised quite differently to its British counterpart. Whereas the RAF had separate Bomber, Fighter, Coastal and Training Commands, the *Luftwaffe* was organised into four (later five) *Luftflotte* (Air-Fleets) that were tied directly to supporting particular Army Groups. Each *Luftflotte* had bombers that operated like long range aerial artillery, attacking troops and communications way behind enemy lines. There were also fighters assigned to protect the bombers. The strategy was evolved during the Spanish Civil War, and was later used to great effect in the invasions of Poland, France and the Low Countries.

A prerequisite for effective use of the bomber aircraft was that of accuracy (a problem the British were also to encounter and face when their thoughts turned to the offensive - see chapter 4). If the bombers were to be useful, they had to be able to deliver their loads very accurately. Whilst this was not so great a problem during daylight, given air-superiority and well-trained crews, night or cloudy conditions would mean no air operations. Consequently the Lorentz Blind-Landing system was adapted for use as a blind-bombing system.

It had features that were similar to the later British OBOE. Two overlapping beams were projected in the direction of the target, such that the area of overlap was not very wide and lay directly over the aiming point. Each beam was a transmitted Morse-code note, one containing dashes, the other dots. The two were timed so that when the operator listened to the received tone, if the aircraft was in the area of overlap he heard a continuous tone, and if the aircraft strayed hearing dots or dashes would tell him whether he was left or right of the centre-line, and therefore which way to move. In order to facilitate accurate bomb release, a third beam was transmitted that intersected with the main beam at a point on the run-up to the target. When the crossing beam was received, it started a clock-work timing mechanism that released the bombs after a certain time. The arrangement of timer and cross-beam were set in conjunction with the operational height of the aircraft in order to ensure that the release of the bombs was timed so they would hit the aiming point.³⁶

This particular system was not a radar. Apart from some experiments into radio altimetry, very little was done at all by researchers in Germany in the area of airborne radar until 1940. This is unsurprising given the nature of the German strategy and war-aims, and the way their research and development was organised. As I mentioned earlier, the prime reason for developing airborne radar in Britain, and also the main impetus for developing centimetric radar, was the threat from attacking bombers. It was only after British strategy became more aggressive that radar became used for bombing purposes. German strategy was aggressive from the outset, and used air bombardment. Furthermore, such bombardment was also essentially tactical (in support of other operations) rather than strategic (an aim in itself). Strategic long-range bombing was the chief spur to British

³⁶ The British were to suffer at the hands of this device, which was used on many occasions during the night-Blitz of Winter 1940-41. The story of its discovery, and British counter-measures to it, can be found in Jones (1978).

bombing-radar development, and this spur did not exist in Germany at this time. Competition and secrecy between firms also didn't aid the situation in Germany. Furthermore by the outbreak of war most firms had close relations with a particular customer for particular equipment that more than satisfied their production capacity. They therefore had little incentive to develop in new directions.

6.4.2 Later Developments up to January 1943

It was only in early 1940, after some experience of war, that the German High Command began to enquire whether airborne radar was practicable or not. Further months passed before any flight tests due to two problems, one familiar to British designers and the other not. Firstly, the German firms who began research in this area had to go through the process of actually constructing equipment that was small and light enough to fit into aircraft, something that had taken their British equivalents a similar amount of time between having the idea and fitting equipment into aircraft. Secondly, the *Luftwaffe* ordered that no antennas should protrude outside the aircraft so as not to impair aircraft speed or performance. This rule was only finally relaxed in February 1941. Such experience was not unique to Germany; Lovell had a similar problem in convincing Handley-Page to mount the external cupola for H₂S on their Halifax bomber.

In 1939, Runge had worked on an experimental radio-altimeter designed to pull a bomber out of a dive at a certain minimum height. However, there was no real official interest and "the thing was far too big to go into an aircraft, so we put it in a shelf in the laboratory and forgot about it."³⁷ In the Summer of 1940 Martini, head of the Luftwaffe Signals, approached Runge with a view to Telefunken building a small airborne radar system for the night-fighters in Holland to use against British bombers. Runge recalled the altimeter system, and decided to adapt it:

³⁷ Runge's memoirs, quoted in Pritchard (1989), p68.

It would of course need a sensitive new receiver with a cathode-ray tube and, above all, a new type of directional antenna. Obviously, even a 1 metre diameter paraboloid with a *Quirl* [rotating dipole] would cause far too much air resistance. Well, to cut a long story short, we re-designed the original equipment and managed to make it much smaller - it was only assembly line work really - and we gave it an air test in August. Its range was only about 4 kilometres, and you could only track one aircraft at a time, and even then it had to be a steadily-flown target.³⁸

It is disappointing that there are no more details of the development of this rudimentary airborne radar. One would expect that they would have had to go through much the same processes that the British went through in designing and building theirs. However, Runge's comments that the re-designing amounted to little more than assembly-line work are interesting. This probably suggests that he had little to do with the work personally, for if the British experience was anything to go by there was a lot that had to be done. For them, building airborne radar required the learning of a lot of practical knowledge about how to build and use airborne radar, and I believe that the Germans would have had to go through a similar process.

The Telefunken engineers installed the equipment, named *Lichtenstein A* in a Me 110 two-seater night-fighter in Autumn 1940, and sent it to a Luftwaffe squadron for trials. This was in principle a good idea, for it was only when the British actually used their AI in combat conditions that they worked out all the limitations of the system, many of which could not have been foreseen, and the best way to improve it and to use it. However, Telefunken appear to have simply dispatched the aircraft and left the Luftwaffe pilots to get on with it, with no attempt being made by the builders to help train operators how to use it effectively.

According to Runge, for the first six months at the squadron the aircraft remained in the hangar, as the mainly aristocratic night-fighter pilots viewed the utilisation of such aids as "unsporting". He only found this out when he questioned Martini about how the device was received. Shortly after his enquiries, a pilot of more ordinary origins was persuaded to use the radar equipped plane. After a period of trial and error he and his observer got to grips

³⁸ Runge's memoirs, quoted in Pritchard (1989), p68.

with how to use the radar effectively (even though its performance was limited). He then successfully shot down several planes, to the point where he was nearing qualification for the Iron Cross, First Class. This was awarded to pilots who reached a certain “score” of downed aircraft. At this point he was grounded by his CO as it was not deemed satisfactory that he should receive such an award after using unsportsmanlike methods. —

Although this story may seem amazing, when viewed in the context of the German military and their War-situation at the time, it is not out of character. Firstly, during the winter of 1940/41 German bombers were successfully attacking British cities using radio methods, and were largely getting through unscathed (significant success with AI did not appear until May, by which point shortening nights and preparations for Barbarossa led to the Germans reducing their campaign against Britain). Conversely, the British bombing campaign during this time was largely ineffectual due to the inability of most navigators to get to their target by using dead-reckoning (see chapter 5). There was, therefore, no significant pressure on the Luftwaffe to get results by shooting down aeroplanes at night in 1940/41 - the Flak batteries appeared to be coping significantly well with such aeroplanes as were coming over from Britain. This sometimes manifested itself in a more chivalrous attitude that did not survive after the Russian campaign and the bombing of cities began in earnest. This contrasted to the British, who felt immediately that their cities were under threat and were pursuing every means possible to stop the German raids that commenced in the winter of 1940.

The other reason for a lack of official interest in radar and other technological advances, apart from the fact that the Germans appeared to be winning the war with what they had, was the attitude of those at the top. Göring, head of the Luftwaffe and number 2 in the Nazi state was a former First World War fighter pilot, and was extremely ambivalent towards technological aids for his airforce. When shown the prototype *Würzburg* he was so pleased he made his infamous and oft mis-quoted remark about adopting a Jewish surname if the Ruhr was bombed. At another demonstration he made a disparaging remark about not liking boxes filled with coils in aircraft. Furthermore he appointed cronies from his WW I flying

days to prominent positions in the Luftwaffe, so the official attitude permeated down the structure.³⁹

On the part of the pilots, this attitude, coupled with their distrust of new devices that ruined⁴⁰ the aerodynamics of their aircraft, meant that they too often did not use the new devices given to them. In chapter 2 I described how when the British AI was introduced the reception for it was lukewarm due to its poor serviceability and a lack of appreciation of how to use it properly, together with the lack of training and other aids to create a proper system of night-time air defence. It is most probable that these complaints existed amongst German pilots too, coupled with the aerodynamics problem which was not as big an issue in Britain due to the different design of antenna. German designers got round the problem of making a narrow beam by using large, directional antenna for their longer-wave apparatus. British designers used a floodlighting method in AI Marks I-IV. Linked with the official attitude to radar in Germany, this mistrust contributed to an extreme lack of interest in air-borne radar that manifested itself in a failure to pursue this direction until well into 1941.

The *Lichtenstein A* was modified by a Telefunken team, and installed for further tests in a Heinkel aircraft. They obtained promising results with an external antenna array, but were forbidden from carrying on these experiments by the Luftwaffe order preventing external arrays. This was largely due to the attitude of the pilots, who refused to fly with the external aerials for the reasons described in footnote 40. After Runge's experiments with an internally-mounted array (called *Sägefisch*) gave extremely poor results compared to the externally-mounted arrays, he finally persuaded Martini to rescind the order in October 1941.

This led to the production of the modified *Lichtenstein BC* in February 1942. It had a very sensitive receiver, and worked on a wavelength of 90cm and a power of 1.5kW. During testing in the September of the previous year, the set, used in conjunction with a ground station, was successfully used in combat to shoot down six planes in one night. The

³⁹ Pritchard (1989), p148.

⁴⁰ This was more than mere prejudice on the part of the pilots. The large external antenna received nicknames like "bedstead", and altered the aerodynamic properties of the aircraft they were mounted on. They could reduce the maximum air-speed by some 30kph, and significantly impaired handling characteristics at low speeds. This had the effect of making low-speed manoeuvres, such as landing, much more dangerous.

display was very similar to the British AI Mark IV, except it also had a range-only screen. The antenna were mounted in left/right, up/down pairs as in the British set.

The only other radar in operation by the end of 1942 was the GEMA *Rostock* ASV radar. In the July of the previous year Atlas-Werke built a prototype ASV set, but little of this is known. Later that year, the Germans captured a Mark I ASV intact in an aeroplane in North Africa (which was based on Mark IV AI, and worked on the same wavelength). German radar of the time worked on a wavelength about half this wavelength, and was generally much smaller and lighter. They dismissed it rather contemptuously with the phrase *viel Luft in den Geräten* (much air in the apparatus), which meant that it had a lot of wasted space. It had no influence on German radar design, except to confirm German suspicions that Allied technology was inferior to their own.

6.5 Conclusions

The Germans' strategic outlook and political situation had a very marked effect on the directions taken by their radar scientists. The aggressive stance that they took meant that their goals were very different from the British one of Air Defence. A consequence of this aggressive stance was that radar, perceived mainly as a defensive apparatus, was not pursued with the same vigour that other projects were.

Unfortunately there is a lack of material for the sort of detailed examination of radar development in Germany that I made of the British situation, but there are some conclusions that I can draw from what is available:

- (i) German commercial firms were the main movers in radar research. This differed greatly with Britain where Government Establishments were the main sources of research. Another major difference with Britain was that the military services settled their own perceived needs by setting up contracts with particular firms. As a result, there was far more secrecy and rivalry between firms and services than there was in Germany.
- (ii) By the end of 1942, German airborne radar was roughly in the same position in terms of types of equipment and in the level of technical complexity and advancement as the British

metric equipment then in service. What the Germans lacked at this stage was a research programme in centimetre techniques. In terms of the utilisation of airborne radar, they lacked about a year's operational experience compared to Britain.

(iii) In certain areas, such as the quality of their engineered equipment, they were ahead of the British. As a result they believed themselves to be a match for, if not better than, the Allies in this area.

At the end of 1942 / beginning of 1943 several major events took place to initiate a German centimetre research programme:

(i) The main one was the discovery of the British H₂S in February 1943. This led them to realise, as I will explain in the next chapter, that other technologies and different directions were possible in radar design.

(ii) In Autumn 1942, the War situation began to deteriorate for Germany, and their economy was reorganised and put in the charge of Albert Speer. He began to change the attitudes that hampered many areas of German production, such as the endemic suspicion and secrecy between all areas of society and the military.

(iii) Another aspect of the deteriorating was situation was the increase in air-raids over Germany. This had the effect this had on forcing a re-appraisal of air-defence and airborne radar. This acted as a further stimulus to the centimetre programme, as the increase in air-raids coincided with the discovery of H₂S.

A Further Counterfactual Intermission

In the next chapter I will describe the situation in German radar research at the point, in February 1943, when they discovered centimetre radar. I will go on to examine how they went about uncovering what they could about this accidental discovery in the form of a captured H2S set. However, it is possible to argue that centimetre research had a negative effect on the German war-effort, rather than a positive one, as it diverted resources away into what became an unproductive direction. This need not have been the case,

Firstly, as I have pointed out and will expand upon in the next chapter, the Germans did not believe centimetre waves to be useful for AI. The set they first captured was used for ground-mapping and navigation by strategic bombers. The Germans went on to attempt to rebuild and copy this set, despite them having no need at his time for such an application. Indeed, the *Berlin* copy had to be redesigned to fit into the smaller German aircraft.

Secondly, their belief that centimetres could not be used for AI was only changed after the discovery of British centimetre AI sets. This only occurred because an aircraft shot-down over the channel crashed on the French Coast. British AI-equipped aircraft were not authorised to fly over enemy territory, and it was only bad luck that presented the Germans with evidence for this application. It is very likely, given their prejudices, that they would not have considered AI experiments worth-while.

Given these conditions it is highly likely that if they had not received the gift of captured equipment, the Germans would not have commenced their centimetre programme. the equipment they had was sufficient for their needs at this time. What they lacked, was the *organisation* to use this equipment effectively. However, by late 1943 German night-time air-defence, faced with the pressure of Harris' area-bombing strategy, had improved to the point that Harris had to call off the night-bombing of Berlin. This was due to the unacceptable casualty rate amongst British air-crews.⁴¹ Given this, the events that I describe in the next chapter were by no means certain to occur.

⁴¹ See Saward (1984).

Chapter 7: German Centimetre Research

7.1 Introduction

In the previous chapters of this thesis I have been working towards this denouement. I have described the origins of radar in both Britain (chapter 2) and Germany (chapter 6). I have described how microwave components were originated in Britain (chapter 3), how they were made into a radar system (chapter 4) and then into H₂S (chapter 5). In this chapter I describe what use the Germans made of their discovery of H₂S, and how this fits in with the questions that I outlined in chapter 1.

In the first section I go into some depth about the state of the German war economy. This differed quite significantly to that in Britain, and it is important to the story of German H₂S replication to identify these differences in order to assess whether they played any part in the ease (or lack of it) with which the Germans accomplished their goals. This section makes considerable use of information from Albert Speer's *Inside the Third Reich*. Speer was in charge of the whole of the Nazi War Economy between 1942-45. His analysis of the German economic situation at that time, coupled with supporting evidence which I have gleaned from my own archive work, is very useful in helping to uncover the pressures on German radar research that were external to the scientists' problems of learning about their recent captured equipment.

In the next two sections I describe the impact of H₂S when it arrived, and then what use the Germans made of it. I assess what the Germans actually learned from H₂S, and from where this knowledge came. I then take this information and use it to address some of the issues in Science Studies that I discussed in chapter 1. In particular, I want to re-examine Collins' ideas on replication in the light of the Germans' experience with H₂S. In this chapter I will highlight instances of copying, and define the difference re-building, copying and replication. I will assess whether the re-building of H₂S by the Germans fits into the categories that Collins defines. In the next chapter, I will assess the findings of this chapter within the terms of the aims set out in chapter 1.

7.2 The Change in the German War Economy 1942-3 and the Reasons for the lack of German Centimetre Research

One of the most striking differences in the circumstances between early British and German radar development lay in the way each side organised the relationships between the Services who required radar, the companies who built it and the researchers who developed it. In Germany there was not the same strategic goal of air Defence to spur the development of radar as there was in Britain. Fear of air attack was, for the British, the main reason for pursuing scientific means of air defence. This culminated in the development of the early-warning radar chain, and, ultimately, centimetric airborne radar. Importantly, radar was always seen as an important means of national survival, and was therefore awarded high priority from the very top levels of government. This had the effect, eventually, of centralising centimetric radar research into one government establishment. There were also many concerted efforts made to improve communication between different organisations concerned with developing, building and using radar, and between different levels within these organisations. It is fair to say that information exchanges, especially in the first year of the war, did not always go smoothly as the case of the Airborne Group in St Athan (see chapter 2) illustrates. Conversely, by 1942 at the height of the H₂S project Lovell was able to appeal almost directly to the Prime Minister, and to the head of his ministry, Sir Robert Renwick, in order to speed up bureaucratic processes that were slowing down aspects of his work.

By direct contrast, the whole culture of Nazi Germany worked in opposition to the free flow of information. As I described in the last chapter, radar development in Germany took the pattern of individual firms supplying the requirements of one Service, and sometimes even a particular branch of that Service. This was compounded by the rivalry, competition and secrecy between the Services and also the firms, such that information was not shared between them. Whilst this may have been acceptable in an atmosphere of peace-time private-enterprise competition, its continuation during war-time seriously hampered efforts to standardise and streamline war-time production, as Speer found out when he took over the running of the economy in 1942. The situation in Industry and in the Armed Forces mirrored that in the Nazi Party, where the over-arching principles were “divide and rule”, and “expand one’s sphere of influence at the expense

of others". Speer, as a confidant of Hitler and a highly placed member of the Nazi government, was well equipped to observe these processes at work.

When the Second World War started, Britain set in motion a whole set of changes to the economy, many of which had direct influences on the development of radar. Some of them, such as setting up AMRE and CVD, were done before the war. Others, such as the employment of University scientists in government research establishments, and enforced co-operation between hitherto competing commercial firms, came soon afterwards. The British Government soon realised that the only way it could fight a war against Germany and maintain its commitment to the Empire, was to engage the whole populace in a policy of total war. A consequence of this policy was the centralisation of planning of production in order to make sure that scarce resources of labour and materials were not wasted in a duplication of effort. It was not always the case that such central planning could ensure swift and plentiful supply of vital components, if, for example, the necessary skills or production facilities simply did not exist in the first place. There were certainly shortages and mistakes were made, and one revisionist historian has reassessed the British war economy as having been a disaster, exemplified by the need to import large amounts of material from the U.S. because British workers and workshops were incapable of supplying sufficient amounts of the right quality at the right time.¹ Another example from a contemporary source showed that as late as January 1942, one of the reasons advanced for pursuing a klystron version of H₂S was that all the available magnetron-production capacity was taken up supplying the requirements for AI Mark VIII.² The previous year, a deciding factor in taking the cavity magnetron to the USA on the Tizard Mission was the realisation that producing enough magnetrons to satisfy expected demand would take every single skilled machinist *in the country*!

Nevertheless, despite difficulties and setbacks, British policy was geared towards maximising co-operation. In some cases, where there was official scepticism or hostility towards certain radar devices which could have delayed projects, the objections were usually over-ridden either by a demonstration or a "Sunday Soviet" meeting (as in the official scepticism over the wisdom of pursuing centimetre research in 1940), or through

¹ Barnett (1986).

² AVIA15/1609, 37a (26/1/42).

pressure exerted by the Prime-Minister through his association with Cherwell (as in the case of H₂S).

In Britain radar research was hampered by economic events, even though there was a culture geared (paradoxically, in wartime) towards co-operation. In Germany the shape of the war economy also had a great effect on the way radar research, especially centimetric research, progressed. This was especially great because war-time conditions enhanced the natural Nazi inclination towards suspicion and excessive secrecy. In this section I wish to explore the impact of how the realities of economic life affected the centimetre research programme firstly, in killing it off in 1940, and secondly, in impeding it when it was restarted in 1943.

Speer has written of how great was the difference in attitude between Britain and Germany over the domestic approach to the war:

It remains one of the oddities of this war that Hitler demanded far less of his people than Churchill and Roosevelt did from their respective nations. The discrepancy between the total mobilisation of labour forces in democratic England and the casual treatment of this question in authoritarian Germany is proof of the regime's anxiety not to risk any shift in the popular mood.³

It took a long time for the German⁴ war economy to become totally geared up in the same way as the British, and there are several reasons for this. Initially it wasn't necessary, but when it was there was resistance to going over to the idea of Total War. However, the main reason why it took so long was because of Hitler himself.

Firstly, prosecution of the war was in the hands of Hitler, as supreme commander. In this, as in his Foreign Policy prior to the war, he had no overall strategy. He acted largely on gut feeling and intuition, which meant that there was no long-term planning in

³ Speer (1993 edn.), p300.

⁴ What Speer is alluding to in the quote, was his belief that in order to maintain support for a war of conquest, Hitler had to ensure that there was no lessening of the material standard of living for Germans. Correlli Barnett, and Speer himself, have argued that the German economy contained a great deal of slack that could have been used to increase production greatly. Speer attempted to do this, and was very successful despite damage to plant from air-raids and a shortage of raw materials. Conversely, the pre-war British economy was very weak, and had to be mobilised in totality to avoid defeat. In some areas it was unable to make up the shortfall, and had to be supplemented firstly by Commonwealth countries, and later by the USA through lend-lease. Barnett, in *The Audit of War*, (Barnett (1986)), is seeking to show that the ailments of modern Britain were created by the wartime economy. His views in relation to the aircraft industry during and after the war are challenged by Edgerton (1992).

any of his moves (unless one counts the aims listed in *Mein Kampf*, including a call for Lebensraum in the East). The speed of victory in the West, and initially in the East, took most people, including the military, completely by surprise. A long war was never Germany's aim. The shrewder military commanders realised that, in the long run, Germany's lack of raw materials would mean eventual defeat when pitched against the combined industrial might and raw-material base of the British Empire and the United States.

Up until the attack on Russia in 1941, the whole German economy was based on small "bursts" of war-activity in conjunction with their military campaigns. In between, the Forces would pause and re-equip, and Hitler would consider where next he should extend his territorial ambitions. This had the remarkable effect of initiating stop-go contracts for war-materials. Ammunition factories would work flat-out before and during campaigns to satisfy contracts, and stand down immediately afterwards. there was no long-term planning to maximise productive capacity in an efficient manner.⁵

Another consequence of this was that the economy never went over solely to producing war-goods. Even as late as 1941, production of consumer goods was only 3% below peace-time rates, whereas in Britain (although not in the USA, which had a far larger industrial base and did not need to maximise production in the same way), production of consumer goods ceased.⁶ The vast majority of workers were men, as women were forbidden from working by Nazi ideology. This meant that many men were working in factories, when they could have been replaced by women (as they were in Britain and the USA) and released to serve in the Armed Forces. Conversely, and equally bizarrely, many highly skilled workers and technicians were drafted to serve in front-line units when their skills would have been better employed in producing war-goods.

That this situation pertained was a combination of the incredibly wasteful Nazi bureaucracy, of a realisation by the Nazi leadership that it was important to avoid breeding conditions for discontent, especially if they were living a highly ostentatious lifestyle in the middle of a war, and of officials "feathering their own nests" and diverting resources to their own pet projects. For example, thousands of construction workers

⁵ Speer (1993 edn.), p301.

⁶ Speer (1993 edn.), p310.

were employed in constructing Hitler's grandiose building schemes, or official residences and concrete bunkers for Party dignitaries right up until the end of the war. Hitler had been around during the period immediately after the end of the First World War when popular discontent overthrew the Kaiser, and later threatened the Weimar regime with a Communist revolution. He was determined to keep the populace happy and subdued. Quick, easy victories and the trappings of peace even in wartime provided this internal stability until the external threat was of sufficient magnitude to cause a rethink in official policy.⁷

However, in February 1942 the stewardship of the German war-economy passed into the hands of Albert Speer, who had hitherto been part of the Nazi hierarchy in the guise of Hitler's Chief Architect. Hitler had a considerable interest in architecture and used Speer as a confidant, which meant that Speer gained a large measure of personal influence with him. Hitler often chose to withdraw from the running of the War at stressful times, such as when campaigns were not going well, and discuss his plans for rebuilding Berlin with Speer.

Up until this point responsibility for running the economy had lain in the hands of two people: Göring, who ostensibly ran the Nazi Four Year Plan for the economy, and Dr Todt. Todt was a civil engineer who rose to prominence before the war by designing the *Autobahnen*. By the time of his death on February 7th 1942, he was responsible for roads, waterways and power-plants within the *Reich*, roads in the occupied territories, and armaments and munitions. He was the head of the Todt Organisation, a labour force of German and foreign construction workers used for major projects. During 1941/2 these workers were constructing the West Wall defences along the European coastline, and U-Boat pens in France, under the auspices of Göring's Four Year Plan. Göring was nominally the second in command of the Nazi state, and in charge of economic policy, although Hitler was always deliberately vague about such appointments in order to ensure no one individual got powerful enough to challenge him. Todt was killed in an aeroplane crash after visiting Hitler's headquarters, following an inspection tour of construction work in Russia.

Todt's replacement was Albert Speer, who systematically set about revitalising the Nazi economy. Because of the profound effect he had on German production it is worth

⁷ Speer (1993 edn.), p301.

taking a look at what he achieved and how he achieved it, to see whether any of the changes he wrought could have had any effect on German radar research and production.

When Speer took over from Todt he was in his mid thirties, and was a trusted friend of Hitler. His rise within the "inner circle" was due to his becoming Hitler's architect, and his responsibility for designing, in conjunction with Hitler, far-reaching schemes for rebuilding Berlin and Nürnberg. During the earlier part of the war he continued to run these building projects, but after the invasion of Russia in late 1941 the scope of his activities widened slightly. His construction duties meant he was in charge of 65,000 workers engaged on the building schemes in these two cities.

As the German armies advanced through Russia between July and November 1941, they conquered huge swathes of territory and created very long supply lines. The German offensive ground to a halt as winter set in, primarily because of the length of these supply lines which caused huge problems for the armies. As the Russians retreated, they ripped up the railways and destroyed all rolling-stock and ancillary railway facilities such as coal sheds, water towers, signal boxes and stations. This meant the Germans had to rely initially on road transport. However, when winter arrived the conditions made road transportation practically impossible, as the roads churned to mud and then froze in deep ruts, and then engines froze in the intense cold. It was imperative to get the railways working again as soon as possible, but this was overlooked in the first flush of victory. Speer heard of the difficulties being faced by the armies in Russia, and recommended to Hitler that he be allowed to divert 30,000 of his workers from the (strictly speaking, non-essential) building projects into the repair of the Russian railways. Hitler took two weeks to come round to the idea, as he did not wish to slow down work on his cherished plans, but finally consented on December 27th 1941. Through the next month Speer made an inspection tour in Russia of the work being done by his men, and was due to fly out with Todt on the same plane, but cancelled after a long meeting with Hitler delayed his sleep. On awaking the next day, he was told the news of Todt's death when the plane crashed, and was summoned to see Hitler. To Speer's astonishment he was appointed Todt's successor in *all* departments, not solely in construction which was the only thing he was vaguely qualified to do.⁸

⁸ Speer (1993 edn.), pp264-5.

On first sight it would appear strange that Hitler should appoint a non-specialist to run such vital parts of the Nazi state during wartime, but:

...it was in keeping with Hitler's dilettantism that he preferred to choose non-specialists as his associates. After all, he had already appointed a wine-salesman as his Foreign Minister, his party philosopher as his Minister for Eastern Affairs, and an erstwhile fighter pilot as overseer of the entire economy. Now he was picking an architect of all people to be his Minister of Armaments. Undoubtedly Hitler preferred to fill positions of leadership with laymen. All his life he respected but distrusted professionals...⁹

By contrast Churchill liked to surround himself with expert opinion, as shown by his close relationship with Lindemann. This could have been a recipe for disaster, but fortunately for Hitler, Speer turned out to be exceptionally good at his new job. Although he was initially appalled at the size of the task that faced him, his experience in two key areas were of great benefit. Firstly, he understood the workings of the Nazi state and was sufficiently "in" with Hitler to be able to get the better of his colleagues, and secondly his experience with running large-scale construction projects had already given him an appreciation of the strengths and weaknesses of the Nazi economy in terms of how things were done and how they might be improved.

No sooner had Speer been appointed than Göring showed his hand. As the number 2 in the Nazi state, and the head of the economic Four Year Plan, Göring felt that *he* should be given command of the armaments programme and the war economy. This illustrated the way Hitler ran his "inner circle"; Göring felt that Speer's appointment eroded his power base and threatened his standing with Hitler. Furthermore, Göring's ostentatious lifestyle was supported by industrialists who used this as a means to get influence in tendering for contracts. On the day following Todt's death Göring tried to ensure that Hitler gave him what he wanted, but Hitler's mind was made up - Speer was in charge. In order to avoid Göring making trouble for him, Speer saw to it that he intimated to Göring that he viewed Göring as his superior. Satisfied that there was no threat to his lifestyle he acquiesced, letting Speer have a virtual free hand. This gave

⁹ Speer (1993 edn.), p280.

Speer much greater scope for making what he saw as improvements to the way things were run.¹⁰

The first official engagement that Speer had to perform in his new role was to attend a conference arranged by Milch, head of the Air Ministry, to discuss the supply of armaments to and between the three Services. It was this conference that convinced Speer that he had to make changes in the current order of things. The main theme of this conference was that bureaucratic incompetence in the form of changing priorities, orders and disputes was significantly interfering with production. In the judgement of many of those attending, there was considerable slack in the economy that was not being taken up for these very reasons. The view of Industry, grudgingly supported by the three Services, was that there had to be a *sole* person to make the sort of decisions that were at that time leading to a state of confusion.¹¹

Speer used this meeting firstly to strengthen his position with Hitler and then to begin the process of tackling the many problems. He had, he felt, an advantage gained through operating at a lower level in the order of things, as he had seen “many fundamental errors which would have remained hidden from me had I been at the top.”¹² The most over-reaching problem between the Services and Industry was the duplication of effort and equipment. An example of this was the different firms contracted to develop specific types of radar by each Service, who had no knowledge of what the other firms were doing (see chapter 6). This policy was deliberately fostered by the Services, who, according to Dr Brandt (Head of Centimetre Development at Telefunken), deliberately organised competition with other firms during peacetime to ensure that Telefunken did not become too powerful.¹³ Amazingly, this situation still pertained in wartime. Immediately after the Industrial Conference on February 13th, Speer wrested a pledge from Milch to end the inter-service rivalry between the *Luftwaffe*, and the Army and Navy over procurement of equipment.

Only five days after his first conference, Speer had a further armaments-related conference in his own Ministry. Immediately preceding this he drew up a set of new

¹⁰ Speer (1993 edn.), p275.

¹¹ Speer (1993 edn.), pp284-5.

¹² Speer (1993 edn.), p287.

¹³ AVIA10/141, Report 5: “Interrogation of Herren Brandt & Kotowski by W.B.Lewis, 24/5/45.” Brandt was, amongst other things, Chief Manager of Development at Telefunken, and an advisor to General Martini, Head of *Luftwaffe* Signals.

priorities for production, standardising as much as possible so that, for example, all ball-bearings were ordered from the same manufacturers. Used to thinking in three dimensions as an architect, he drew out a three-dimensional schema of the new organisation to show to the heads of Industry and the Forces in order to explain how he was changing things.¹⁴ The conference duly endorsed his methods, and on March 21st 1942 he was given sole authority in a decree by Hitler.

Speer's approach owed a lot to a man called Rathenau, who had planned armaments production during the First World War. One of his assistants now worked in Todt's office, which Speer inherited, and was able to advise him on details of what Rathenau believed. Rathenau applied the principle of "Industrial Self Responsibility", whereby:

...considerable increases in production could be achieved by exchange of technical experiences, by division of labour from plant to plant, and by standardisation. As early as 1917 he declared that such methods could guarantee "a doubling of production with no increase in labour costs."¹⁵

Amazingly, these practices, fairly standard for modern production line methods, were not current in German factories at this time. As well as the limited contracts associated with the stop/start nature of campaigns mentioned earlier, very often each plant would be working on four or five contracts for different services at the same time. Speer took steps to ensure that each plant would handle only one product. In the light of the aims of this thesis it is also interesting to note the call for an "exchange of technical experiences" as a means of speeding up production. Speer did not elucidate further, but I take this to mean that he wanted engineers to engage in face-to-face co-operation.

Speer envisaged giving plant managers greater freedom to set and meet their own targets, and to enjoy a flexibility that had hitherto been denied them. For example, at Telefunken during the war it was impossible for the research staff to do their own research independently in areas which interested them. Officially they were only supposed to pursue research that related directly to a military contract. Later they got around this by doing some projects which fell into grey areas.¹⁶ Speer's approach bore

¹⁴ Speer (1993 edn.), p288.

¹⁵ Speer (1993 edn.), p292.

¹⁶ AVIA10/141, Report 5.

fruit in that six months after he started on this programme, production of all armaments had increased significantly. However, at no stage did it ever reach the levels attained during the First World War.

Speer continued to try to implement changes that he felt would improve production. He was certainly successful in these aims, as several sources testify.¹⁷ Furthermore, in many areas German production reached its peak at the *height* of the Allied bombing campaign. Allied bombing was supposed to severely impair German productive capacity. Speer also believed that the Allies' other intended aim, that of destroying morale, failed to happen too. This is interesting, for it is clear that the Allied aims of destroying Germany's production capacity and morale were far from reached. Speer believed that some actions, such as the Dams Raid in May 1943 and the Schweinfurth raids against ball-bearing factories in October of the same year came close to achieving the aims. However, a failure to repeat the attacks allowed these installations to resume production. It is not clear why the targets were not re-attacked, except that casualties were so high in both cases that maybe it was thought that the propaganda value, especially of the Dams raid, outweighed the actual level of achievement.¹⁸

However, the Allied Bomber Offensive had other unforeseen consequences on the war, which were less quantifiable. The British night-time air-raids tied up large parts of the German war-economy. For example, 50% of the German electronics industry was producing radars and communications equipment for air defence in 1944. Ten thousand guns were deployed defending cities rather than on the Eastern Front, as were several tens of thousands of troops to man them. Many more thousands of men were employed in emergency services engaged in dealing with the immediate and medium term effects of bombing, such as fire and ambulance workers and construction/demolition. It is very difficult to gauge what effect these resources would have had if they had been deployed elsewhere, but it is possible that they could have been enough to tip the balance in some of the decisive battles that Germany fought.¹⁹

¹⁷ See, for example, figures for production quoted in Seward (1984) and Webster & Frankland (1961).

¹⁸ Following the fiftieth anniversary of the Dams Raid, there has been a reinterpretation of the effectiveness of the raid. Historians now believe that the value was largely propaganda. Only the destruction of the earth-banked Sorpe Dam, which failed to occur, would have had the effect sought - that of stopping steel production in the Ruhr. BBC documentary, screened in May 1993.

¹⁹ Speer (1993 edn.), pp381-2.

7.2.1 The Effects On the Electronics Industry

Speer's reforms had only limited effect on the electronics industry. On the positive side, Milch's discussions with Speer led to the appointment of General Martini to responsibility for radar for the *Luftwaffe*. He enthusiastically pursued contacts between that service and all the companies and establishments supplying it with radar equipment, in order to facilitate improvements in getting what the *Luftwaffe* wanted.²⁰ Unfortunately, there was still official indifference and ambivalence towards new technology. This attitude came right from the top, as Speer explained:

Hitler's technical horizon... was limited by the First World War. His technical interests were narrowly restricted to the traditional weapons of the army and the navy. In these areas he had continued to learn and steadily increase his knowledge, so that he frequently proposed convincing and usable innovations. But he had little feeling for such new developments as, for example, radar...²¹

In chapter 6 I recounted how Göring, too, had an ambivalent attitude towards radar. Milch shared this attitude, and according to Brandt (a Telefunken engineer), both of them disliked Martini. In true Nazi Party style, they replaced Martini with Dr Rottgart of Telefunken in February 1944. Rottgart was a Party man who was more suppliant to their wishes. Brandt complained that the new organisation became unwieldy.

Speer was especially concerned about the muddle over the conscription of personnel with considerable technical experience into the Services, rather than engaging them in research. For example, by the beginning of the war over 60% of all students had been called up. Furthermore, the number of electronics students fell by 50% over the period 1932-9. This was partly due to conscription, but partly also because of the lack of lecturers due to racial persecution.²²

Speer was also concerned about the low accord given to fundamental research. The latter state of affairs was the legacy of the short-termism affecting German thinking, which had manifested itself in an October 1940 decree from Hitler forbidding the pursuit of any research that would not come to fruition within a year. This order had the effect

²⁰ AVIA10/141, Report 5.

²¹ Speer (1993 edn.), pp323-4.

²² Krcm (1994), p179.

of putting another nail into the coffin of German centimetre research pre-1943, of which more shortly. In May 1942 Speer met with General Fromm, in charge of conscription, who arranged for him to meet with German scientists engaged in nuclear research. On the strength of this second meeting, where, amongst others, Heisenberg complained of the lack of personnel available for this research, Fromm offered to release several hundred conscripted scientists from military service.²³ In order to raise the profile of science, Göring was persuaded to take over as head of the Reich Research Bureau (*Reichsforschungsrat*, *RFR*) in June 1942.²⁴ However, these efforts did not bear fruit in terms of the electronics industry, as archived letters confirm. In August 1943, well into the beginning of the centimetre research programme, Professor Esau of Telefunken wrote to Plendl (Head of the High-Frequency Research Office of the *RFR*) urging that physicists should not be withdrawn into the *Luftwaffe*,²⁵ and a further letter in September 1943 asking for an increase in personnel.²⁶ This situation *still* pertained in 1944, as a lecture by Brandt at that time indicated:

Der menschensatz, nicht die technische Durchführbarkeit bestimmte Tempo und Breite der Einführung der neuen Technik. [The speed and scope with which this technique will be introduced will be decided by the manpower situation and not by technical ability to carry it out].²⁷

Other difficulties inherent within the Nazi system, such as the obsessive secrecy, were extremely difficult for him to eradicate. The reflex behaviour of the past ten years could not be forgotten overnight. Speer tried to see that committees were formed, where ideas about how to improve production and eradicate mistakes could be discussed. There was considerable resistance to these on the part of factory owners, who were conservative towards change and fearful of criticising and being criticised, so they were of only limited success.²⁸ A report on the phenomenon of secrecy, in conjunction with the employment of *Hochschule* (polytechnic) personnel, for the *RFR* written in June 1944 concluded that:

²³ Speer (1993 edn.), pp315-6.

²⁴ Speer (1993 edn.), p315.

²⁵ R26III/132, letter 22/8/43.

²⁶ R26III/132, letter 3/9/43.

²⁷ RL39/515.

²⁸ Speer (1993 edn.), p297.

- (i) Personnel in the *Hochschulen* wish to help with military research, but have no idea what is going on in this area due to excessive secrecy.
- (ii) There is a lack of communication in this area, and consequently a duplication of work in certain projects.
- (iii) Things are often treated as secrets which shouldn't be.
- (iv) There was a lack of co-ordination between firms on industrial processes.
- (v) Material which the enemy already has a knowledge of should be made available for general release to the press and public. This would have the added benefit of countering Anglo-American propaganda about a lack of open-ness in Germany.²⁹

These difficulties were *still* a problem in June 1944, so they go to show that Speer's efforts were only partially successful at eradicating some of the Germans' problems

Hitler's order of October 1940 forbidding research was only one of the reasons why centimetre research failed to get under way in Germany before 1943. In the previous chapter I mentioned how research done at this time gave researchers the impression that the reflection of high-frequency waves by aircraft and other targets would be minimal, and so this line of research should not be continued (a similar thing nearly happened in Britain, see chapter 4). One author neatly summarised what were the reasons for Germany losing the initiative in high-frequency research:

- (i) Hitler, Göring and Milch's ignorance.
- (ii) Rivalry between the Navy and the *Luftwaffe*.
- (iii) Rivalry between the Technical Office of the *Luftwaffe*, the Signal Service, and the Night-Fighter Service within the *Luftwaffe*.
- (iv) Production capacity was one tenth of the Allies.
- (v) There were nearly a hundred small research laboratories which were not allowed to exchange practical knowledge and expertise.
- (vi) Many engineers and craftsmen were serving in front line units rather than doing research.
- (vii) Until 1942, there were no manuals and no training schools for radar.³⁰

²⁹ R26III/140 Report on Secrecy, June 1944.

³⁰ Aders (1978), p74.

This bears out what I have written in the previous chapter, and what I have quoted from Speer in this chapter.³¹ But he also added:

Last but not least there was the long-lasting erroneous evaluation of the centimetre waveband and its usefulness for location and orientation purposes that led to the German inferiority in electronic technology.

In November 1942, only a few weeks before the discovery of the first British centimetre navigation radar in a shot-down bomber, the chief of Telefunken Laboratories, Dr Runge, said that very little could be achieved with centimetre waves, and that only at great expense. Thereupon the centimetre research laboratory was disbanded to concentrate all available technicians on further development of the 50cm and 2.4m wavebands, of which much was expected. This decision shows clearly that the research establishments and the industry had no idea what the fighting units really required...³²

Runge, as head of the largest electronics firm in Germany, would have had considerable influence over what research was carried out by his firm. This was another shortfall of the policy of leaving research to autonomous firms, and the culture of authority prevalent in Germany. The British were not without similar instances, though, as the experience of Rowe and Bowen (see chapter 2) also illustrates.

The above quote illustrates that the kind of lessons that the British learnt early in the war, and especially during 1940 with the implementation of the early 1.5m AI equipments (see chapter 2), were still to be learned in Germany. A lack of communication between the user (the RAF) and the designers (AMRE) led to confusion over what was wrong with the equipment. This resulted in the minimum range fiasco. Subsequently the RAF became more involved with the inception of radar devices, but often in practice AMRE/TRE had to do a certain amount of “selling” of their devices. This process was eased by the later adoption of Operational Research.³³

³¹ The British instituted training for radar operators very early on in relation to Chain Home, in 1938/9. The lesson of the necessity to train operators and mechanics was reinforced with AI Marks I-III in the winter of 1939/40, see chapter 2.

³² Aders (1978), p74.

³³ See Air Publication 3368 (1963) for retails of how Operational Research was originated in Britain. See also chapter 4.

Runge's beliefs were due to the research, related in chapter 6, that claimed to show that reflection from aircraft when illuminated by 10cm would be minimal. Brandt's immediate post-war interview with British Intelligence confirmed this:

Mr Brandt made an interesting statement on why German research did not proceed with the development of centimetre radar until 1943, when H₂S arrived. In spite of disagreement from a number of people, Dr Runge, an eminent and much respected engineer and scientist, claimed to have proved it was not worthwhile proceeding below 20cm because the equivalent number of dipoles for an aircraft did not go on increasing proportionally as λ was reduced. He based all his arguments on this theoretical curve he produced. Somebody later found a slip in the analysis he had employed [see overleaf for diagram].³⁴

This idea that centimetre waves might be mirrored away rather than reflected was upheld by Dr Esau, too, as Pritchard related.³⁵

Runge's scientific, political and social status within the Nazi hierarchy as "an eminent and much respected engineer and scientist" meant that although others may have disagreed with his conclusions, the issue of the non-reflection of centimetre waves was decided in his favour. He had a higher status than those who disagreed with him and this meant that he was able to carry the argument. Happenings like this were a particularly prevalent facet of Nazi society, where deference to authority was regarded as the norm.³⁶ As there was no scope for dispute within the firm, and Runge had produced a graph which "proved" his case, it was only the later discovery of an artefact that appeared to refute his argument (the H₂S set, see next section) that allowed the debate and enquiry to be reopened.

In the next sections I shall outline how the Germans dealt with this particular discovery that refuted the accepted "truth" about centimetre wavelengths and radar. I have explored the background to the German war economy because, in order to make a comparison, it is necessary to consider *all* the areas of science and society that differed

³⁴ AVIA10/141, Report 9.

³⁵ Pritchard (1989), p87.

³⁶ See Collins (1985), ch 4 for discussion of how social processes affect the closure of debate in instances of controversial science.

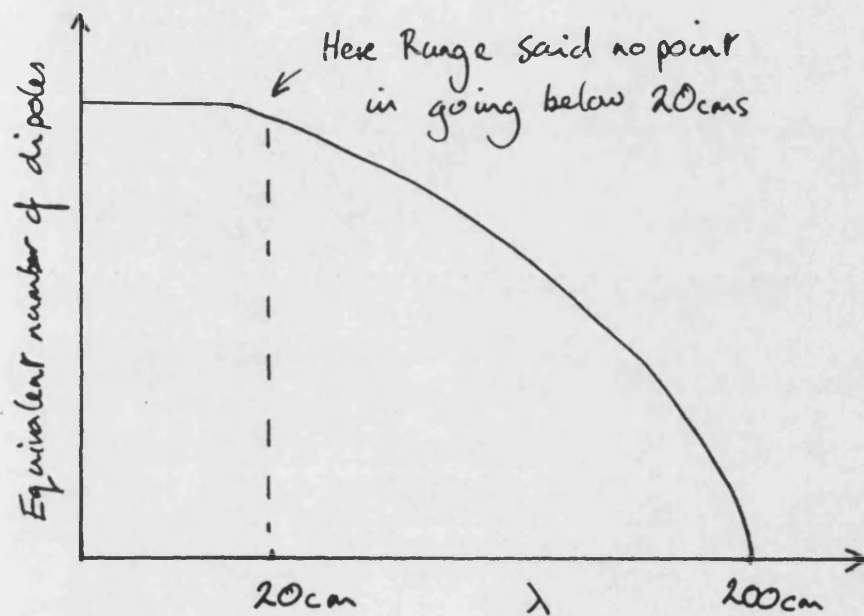


Figure 7.1: German curve relating wavelength to equivalent number of dipoles.
this curve was used to assess whether going to shorter wavelengths was beneficial.
From AVIA10/141.

from Britain. In particular, support for centimetre radar in Britain was always forthcoming and at a very high level.

The situation in Britain differed greatly from the German experience, as I have shown in this section by examining Speer's account, confirmed by several other sources (some original):

- (i) support for radar projects in Germany was often, at best, ambivalent and at worst downright hostile. This was often the case with senior members of the Nazi Party, like Göring.
- (ii) The Germans suffered greatly from conscription of their key personnel. In Britain, key scientists were identified pre-war and recruited into government research laboratories.
- (iii) due to obsessive levels of secrecy, there were always difficulties in communication between different levels in an organisation, and between organisations.
- (iv) finally, towards the end of the war, the Germans suffered an almost complete collapse of their state.

All these points should be born in mind when viewing what happened to German centimetre research, as they made the German experience very different to that of the British.

7.3 Initial Reactions to the Discovery of H₂S

On the night of February 2nd/3rd 1943, one of the British Pathfinder H₂S-equipped Stirling Bombers, on only the second mission of the new equipment, was shot down over Holland. There were at least two other captures, later in 1943, of equipment that was more intact than this, but this was to prove the most significant because it forced a complete rethink of German radar research. It was standard practice for teams of electronic engineers from firms like Telefunken to strip any aircraft wreckage in order to examine the equipment it carried. The H₂S set was discovered, and taken to Telefunken in Berlin for assessment. The one biggest fear for the British about using the magnetron in H₂S was that it was the only magnetron-equipped device that would have any possibility of enemy capture, for its whole purpose was as a navigation aid over enemy

territory (see chapter 5 for details of the discussion over this). The other magnetron-equipped radars were either used only within or above British soil, or else were installed on ships. Assessments by British experts of the time they predicted that it would take to copy the magnetron varied from 3 months to 2 years.³⁷ In the end, the most persuasive argument for using the device was that it would help them to destroy the German's industrial base, and that even if it were not used there was every possibility that the Germans would develop it anyway.³⁸

The Germans possibly had the opportunity to develop the cavity magnetron before their discovery of a British one in 1943. The Japanese had begun researching into split-anode magnetrons in 1937. Their work led them to build a 10cm ship-search radar. The prototype became operational in October 1941, and 100 production sets were delivered by June 1942, as one of the engineers who worked on it related:

The No.22 radar ($\lambda = 10\text{cm}$) had a capability of detecting surface targets at a distance of 35km and proved the importance of 10cm radar, but it had no capability of detecting an aircraft as the antenna was designed for horizontal rotation only. The navy staffs considered air defence to be more important and for this purpose only the meter wavelength was necessary. Further, we had no information that microwave radars were being used by England, the USA and Germany... However... we, the radar engineers, had confidence that the principle of radar is the usage of microwaves. Therefore, we did not stop our research although there were many criticisms.³⁹

Furthermore, as early as 1941 the Japanese had developed a sealed-off cavity magnetron, which bears a striking resemblance to the Randall/Boot device of the previous year (see overleaf for picture):

We... completed a prototype on an all-metal magnetron in 1941, but we could not proceed any further due to the shortage of materials for the permanent magnets and of manufacturing facilities.⁴⁰

³⁷ Batt (1991), p107.

³⁸ Saward (1984), p155.

³⁹ Nakajima (1988), p249.

⁴⁰ Nakajima (1988), p255.

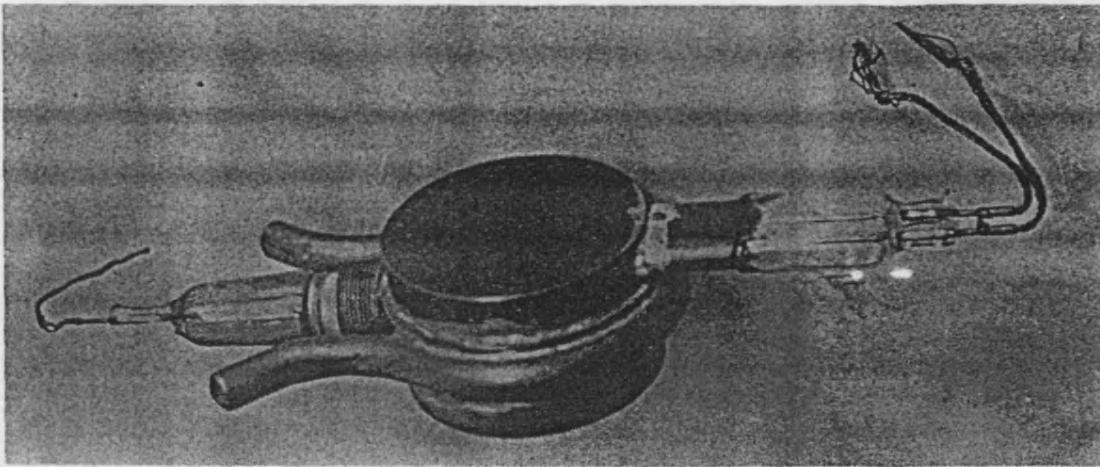


Figure 7.2: Japanese cavity magnetron, 1941. Compare this to the British magnetrons, figures 3.2, 3.3, 3.4. From Nakajima (1988), p256.

However, the level of information exchange between the Axis powers was far below the norm of that for the Allied powers, where British revelations to the Empire and to the USA paved the way for full information exchanges between these countries. This did not always guarantee good results (such as the problems over whether the Americans could make H₂S work - see chapter 5), but it did at least avoid the sort of situation where something as major as the cavity magnetron could be invented in one country and unheard of in one with which it was supposed to be co-operating.

The problem was that the Germans generally believed themselves to be technically superior to other nations, which often meant that they failed to take note of other countries' innovations. Therefore, even if the Japanese had informed them of their cavity magnetron, there is no guarantee that they would have used the information. This was certainly the case with the Russian cavity magnetron, which was invented in 1939 and published freely (albeit in a Russian journal). One German researcher, under interrogation immediately after the war, indicated that published material on the Russian cavity magnetron reached Germany in 1940 but that no interest was shown until the discovery of the British version in 1943. The same source said that the Germans viewed Italy as suspect, and technical liaison between the two countries was not close. There was some liaison with the Japanese, but they "gave little away, even if asked", and besides the Germans thought they had nothing to learn from them⁴¹. A German Würzburg radar was sent to Japan for licensed production in 1943, but according to a Japanese researcher this was the sole co-operative venture between the two powers.⁴²

The immediate German reaction to their discovery of the H₂S set was to set up a committee to discuss the implications of the find. They realised very quickly that the equipment was a centimetric device. As the device was discovered near Rotterdam, it was given that name, and the committee became the *Arbeitsgemeinschaft Rotterdam (AGR)*. The committee minutes form a useful source of information about what the Germans thought of and did with their discovery.

One of the important questions to ask at this point, is how did the Germans realise that the device that they had been presented with was a centimetric device? The *AGR* minutes do not give any detailed information on this point, and I have been unable to

⁴¹ AVIA10/142, Report 14.

⁴² Nakajima (1988), p257.

locate any other information that might give the same level of detail that I have been able to find about the British experience (sadly, Telefunken were unable to furnish me with any information as their archive was destroyed or dispersed after the war). However, we can conjecture how they might have come to this conclusion.

The first point to make is that the H₂S set was examined by German electronic *experts*. The Germans were not working in a vacuum, they were part of the general Western scientific culture that had shared beliefs and equipment up until the outbreak of the war. As such, the German electronics experts would share a lot of common knowledge about the usage of certain types of electronic components. Secondly, as the H₂S set was stalled in a bomber aircraft, they could make intelligent guesses about the purposes of the equipment from this information.

H₂S contained several components that were new to the Germans, such as the reflex klystron and the cavity magnetron. However, it also contained many components that would have been familiar to the German engineers. What this would have told them was the wavelength that the equipment worked at. They knew what waveguides were for, and they knew that the dimensions of conventional glass-envelope valves were in proportion to the wavelength that they were used at. The sort of information that they could have gleaned from this was readily available as it was knowledge that was culturally shared.

However, there is another important point to make here. The Germans were able to work out on initial inspection that the device worked using centimetre waves by virtue of their inclusion in a common scientific culture with the British scientists who built the device. But what the H₂S set could not tell them on a cursory examination, was the local, tacit knowledge of the scientists that had gone into solving the particular problems that they had encountered in getting H₂S to work, and in operating H₂S successfully in its intended role. In the next section I will analyse in greater depth what exactly the Germans *were* able to learn from the artefact they had received.

The first meeting of the *AGR* was convened on February 22nd 1943. Most of the senior figures in German radar were present, from the military, and from the companies and research interests. At this first meeting, the main problem that was tackled was the allocation of research contracts amongst the various firms. Telefunken, as the largest company with the most “clout”, got the lion’s share. It shows how the fact that the

Germans had no central organisation for doing this sort of work (as the British did) meant that they had to settle questions such as this before actually getting down to the problem of investigating the equipment.

One of the first difficulties that they faced was that the British were using polythene cores for their coaxial cables. The Germans did not have the resources for making this plastic which was, at the time, very new. A further difficulty was that they were still unsure of what the device was actually for, as both the display unit and the scanner had been destroyed when the bomber had crashed. As they only had a limited amount of information to go on, they initially surmised that it was a detector for night-fighters. They also decided to develop a detector for 10cm radiation which they could install in aircraft and submarines. This was to be named *Naxos*.⁴³ They realised that if British bombers were broadcasting 10cm radiation and they could detect it, they would be able to either chase British bombers (in the case of German fighter-aircraft) or avoid them (in the case of German submarines). They also took the decision to try and re-build the set. Re-building, in this case, was *not* the same as replication. I take replication to mean building a copy of the equipment which may differ from the original slightly. In this case, the Germans wanted to use the apparatus that they actually had. The H₂S set was not complete, however, so they would have to check all the circuits to find any damaged components and replace them with either copies or versions of their own components. they would also have to try and work out which bits if the equipment were missing, what the missing pieces were for, and then try and build something to take their place. On the face of it, it was a tall order.

When Göring heard of the discovery, soon after the crash, he remarked that:

We must admit that in this sphere the British and the Americans are far ahead of us. I expected them to be advanced, but I never thought that they would get so far ahead. I did hope that if we were behind we could at least be in the same race.⁴⁴

One suspects that some of the more far-sighted German engineers would have been irritated by such remarks, given Göring's attitude to radar and the fact that much research had been cancelled (especially that in centimetres). One positive thing about the

⁴³ Brandt (1953), 22/3/43.

⁴⁴ Pritchard (1989), p88.

discovery was that it demonstrated to the Germans that 10cm radiation must be useful for something, so it gave a much needed boost to the moribund German radar industry, which was riven at this time by internal disagreement over the reopened and recently closed dispute about centimetre waves. Ironically, just as investigations into *Rotterdam* began to get underway, the RAF bombed Berlin on 1st March. During this raid the Telefunken works, including the captured H₂S units, were severely damaged.

At the next meeting on March 17th, the investigations had progressed. Through the interrogation of a POW they had learned that the captured equipment was used both for ground-mapping, and for blind-bombing. They believed (correctly, although they had no direct evidence for this) that the sets were used by Pathfinder⁴⁵ aircraft to drop target-markers for other aircraft to aim at. It is important to note that they needed the testimony of someone who actually *used* the device to confirm exactly what it was *for*. However, the bomb-aimers using the device would not be familiar with the construction of the device, so they would not be able to reveal tacit knowledge about how to *build* one.⁴⁶ Also, because the scanner had been destroyed, they missed the second feature of the set (apart from the centimetre wave usage), that it used a PPI presentation with a rotating scanner. At the same meeting Dr Steimel revealed that the magnetron gave out 9.15cm radiation at a power of between 20 and 30 kW, a power massively higher than anything available so far in Germany at the same wavelength, and that the efficiency was around 10%. The wave output was very stable, the frequency not varying significantly. Arrangements were made with the firm Sanitas to build copies of the valve for the German rebuilt *Rotterdam*.⁴⁷

Apart from the question of how they actually managed to re-build and then copy H₂S, another important question we should ask, is why would they want to copy it in the first place? H₂S was a bombing radar, and the Germans did not possess any long-range bombers. Indeed, they possessed very few aircraft big enough to carry the massive rebuilt H₂S equipment at all:

⁴⁵ See previous chapter for a description of Pathfinder tactics.

⁴⁶ Aircrew survival rates from damaged aircraft were only in the region of 20%, so it is remarkable that they were able to get any information from captured RAF personnel so quickly, given also that H₂S was only installed in a very small percentage of aircraft at this time. AP3368 (1963).

⁴⁷ Brandt (1953), 17/3/43.

[The Germans had] a curious mixture of admiration for the British technique and a low opinion of its actual construction. To the German designer it was a constant source of wonder that his British counterpart used such (comparatively) large components on a large chassis and put the lot into an even larger cabinet. Such profligacy offended his sense of order and compactness.⁴⁸

The capture of another, less damaged apparatus in May 1943 helped considerably, to the extent that by the June 1st meeting Herr Maas was able to say that they had learned new technical details. He was able to reveal that the [local] oscillator was a type of klystron, and the antenna used 3 dipoles; information unavailable from the first equipment because it had been destroyed. By this meeting the reconstruction of all the electronic units was completed, and work had just begun in installing the units into an aircraft. The twelve H₂S units⁴⁹ are shown below:

Heading Control Unit	Scanner	Modulator	Alternator	Waveform Generator	Receiver & Timing Unit
Indicator Unit Tp. 162	Voltage Control	Switch Unit	Transmitter	Tuning Control Unit	Heading Control unit

Dr Brandt was now to prepare a programme for flight testing.⁵⁰

Apart from the reconstruction programme, the discovery of the existence of centimetre waves as a viable option for certain types of radar led the Germans to restart fundamental research into the area. They were not starting from the beginning, because very short wavelengths had been the original choice some ten years earlier for Runge. Despite the various subsequent official orders prohibiting any research in this area, one or two researchers at Telefunken had maintained an interest.

The other line of development which came out of the discovery of H₂S at this was the Naxos series of passive warning devices. Naxos was one of the first applications envisaged after the discovery of H₂S, as I explained above. The Germans realised that any device mounted in a bomber that broadcasted powerful centimetre waves, would

⁴⁸ Pritchard (1989), p91.

⁴⁹ AVIA26/487, 5/7/43.

⁵⁰ Brandt (1953), 1/6/43.

present them with a means of passive detection of British bombers. However, by mid 1943 the prototype centimetre receivers had not progressed to the stage of flight testing.

7.4 *Rotterdam and Berlin*

The decision to re-build *Rotterdam* had a profound influence on German thinking in relation to how to use centimetre waves for radar. As I mentioned earlier, the H₂S apparatus was bulky and heavy and designed for use in British four-engined heavy bombers. It consisted, in all, of twelve separate units as I detailed on the previous page. The Germans had to install the first re-built *Rotterdam* into an aircraft that was roughly equivalent to the British bombers. The only suitable one was the *Condor*, a large four-engined aircraft designed for long maritime patrols. However, the *first* time the whole re-built set was made to work was from one of the Flak-Towers⁵¹ in Berlin. The rotating scanner and PPI picture underlined power of the device, as the scanner showed clear returns from prominent buildings on the Berlin sky-line. This initial viewing was meant to give them some indication as to what they might expect from the device when it would be finally flown. However, what they *actually* saw, as the British found when they first flew centimetric AI (see chapter 4), was a great surprise, for the device was far more powerful than they had believed would be the case.⁵²

At this stage, in the summer of 1943, there was still a belief by people such as Runge that centimetre waves were unsuitable for detecting aircraft. They still thought that the waves would be mirrored away from smaller objects, rather than reflected and detected as they were with H₂S. No British cm AI sets had been captured by this time, so they still assumed that the only British airborne radar was H₂S. On the basis of these beliefs the *AGR* took the decision not only to *re-build* the captured British equipment, but also to redesign it as something smaller, lighter and more suitable to German bombing needs. Whether this particular application was the most appropriate to their *overall* needs, when

⁵¹ The Flak Towers were large concrete buildings, some ten storeys high, that had Flak batteries and search-lights mounted on their roofs. The buildings were bomb-proof and provided accommodation for the Flak personnel. They were built in response to the increase in bombing that began in 1942 when Air Chief Marshall Sir Arthur Harris was appointed as head of Bomber Command, and the US entered the war.

⁵² Brandt (1953), 22/6/43.

the war situation for Germany was turning towards the defensive, is debatable. However, talk of military defeat was tantamount to treason, and it was only the following year after the decisive battles on the Eastern front and the Invasion in the West that the more shrewd Germans began to realise that the war was turning inexorably against them. In 1943, Hitler still dreamed of eventually bombing America. Nevertheless, the June meeting also approved the commencement of a programme to build a wholly-German centimetre radar, named *Berlin*. It was to use the German copy of the British magnetron, named LMS10, and to consist of four units⁵³ (as oppose to *Rotterdam*'s twelve):

High-Frequency Head (incl. Transmitter Unit)	Pulse Generator and Low-Frequency Unit	Receiver & Viewing Apparatus	Power Supply
--	--	---------------------------------	--------------

Berlin was to be smaller and lighter than *Rotterdam*, in order to be fitted in Germany's smaller, lighter twin-engined bombers.⁵⁴

At the July meeting the AGR was told that a programme for research on the airborne *Rotterdam* had been agreed. The purpose of the flights were to test its performance against various types of terrain and also to see if it could detect ships and submarines. Four LMS10 magnetron copies had been manufactured by this meeting, and it was reported that in a short time they expected to be making 10 per week.⁵⁵

In the summer of 1943, a British AI Mark VIII set was recovered from a Mosquito night-fighter shot down over the Channel coast. This was unfortunate for the British, as they had made a policy decision not to fly the cavity magnetron over enemy territory (apart from in H₂S). The Germans interrogated the radar operator from this aircraft in order to find out the purpose of the set. He gave them details of the set's capabilities, such as its range.⁵⁶ It is unclear how the Germans actually extracted this information from their prisoner. Under the terms of the Geneva Convention, prisoners were only

⁵³ RL39/515.

⁵⁴ Brandt (1953), 22/6/43.

⁵⁵ Brandt (1953), 23/7/43.

⁵⁶ AVIA26/1154.

required to give their name rank and number, and airmen were not supposed to give any information about the equipment they carried. It is possible that he was tortured in order to obtain the information, but he may just have answered the questions the Germans put to him. The British used to obtain information about German equipment by listening to prisoners' conversations, when the prisoners thought they couldn't be overheard. In this way R.V.Jones was able to gain information about the Knickebein blind-bombing system (see chapter 6 for details of this system).⁵⁷

By the meeting on 9th September, the AGR reported a *Rotterdam* had been installed in a Heinkel He111 and was being used for flight-testing of the apparatus. After a short period of testing, they were amazed to find that not only did coastlines show up, but also differences between built-up areas and open countryside. Later that year, as the Allied Bomber-offensive on Berlin heightened, the Germans' thoughts turned away from their offensive-oriented ground-mapping usage of *Berlin*. They decided to look for other means of employing centimetre radar in a defensive manner apart from the aircraft-mounted receiver, *Naxos*. Incredibly, despite the discovery of the Mark VIII centimetric AI, there were *still* sceptical voices as to whether centimetre waves could ever be used to locate aircraft. This was a clear instance of sceptical theorists challenging practical scientists. In order to find out if aircraft detection was possible, Telefunken engineers installed Berlin units into a Würzburg radar. At the same time as a "high-frequency physicist" was explaining that they would be lucky to get ranges of 8km on aircraft, this hybrid equipment was giving ranges of 30km.⁵⁸ Unfortunately, Reuter did not say who this physicist was, but it is comparable to the situation that Dee faced over whether to continue with centimetre AI in 1940 (see chapter 4). The men working on the equipment, by virtue of their practical knowledge of how to get the best from it, were able to make it perform far better than the theorists predicted.

On February 8th 1944, Brandt gave a lecture to an audience of high-ranking *Luftwaffe officers*, including Field Marshall Milch (Air Minister). The purpose of this lecture was to give an overview of the state of affairs in centimetre research at this time, and to confirm the shift in policy away from the offensive utilisation of *Berlin* in a ground-mapping radar for bombers towards applications that were more defensive.

⁵⁷ Jones (1978).

⁵⁸ Reuter (1971), p117.

These included the Naxos detector, various ground-based early-warning radars, and a variant of *Berlin* to be used as an AI radar. As I mentioned in the last chapter, German engineers had to a certain extent solved the problem of the ground-return and AI by using large, externally-mounted antennae on their night-fighter aircraft. However, although this meant that the maximum range of their AI sets was comparable to British centimetre AI, the aerals were unpopular with the pilots as they impede the performance of the aircraft.

Brandt's lecture is a remarkable document. It outlines that some Germans had realised just how far they would have to change their methods of research if they wanted to compete in the centimetre-technology war. Brandt showed a considerable degree of awareness of what the British had to go through when they develop H₂S, as he described to his audience how he believed events had happened in Britain:

In the early stages they worked with comparatively incomplete equipment. They used equipment which was very much in its experimental stages and had to overcome all kinds of "teething troubles" connected with equipment and technique. In the early stages, technicians were sent out on flights because the fundamental significance of the project was fully appreciated. They recognised that radar was the eyes of the fighting units and that these sets were at their best when their wavelengths were nearest to light waves.⁵⁹

He highlighted that radar was a project of "fundamental significance", because the Germans had had many difficulties in introducing radar to service against the opposition of the men who had to use it. The case of Lichtenstein clearly illustrates this point, although their opposition was hardly surprising given the way the large and obvious external antennas hindered their night-fighters' performance. In order to tackle this problem, Brandt argued for the introduction of an approach very similar to that taken by the British with their Operational Research:

We must realise that in the course of introducing the *Berlin* equipment, we shall also have to employ an increasing number of technicians; we are in complete agreement with the *Luftwaffe* Technical Control Office that failure in this respect would be a grave mistake, the result of which would be reluctance on the part of our

⁵⁹ AP1136, p631.

forces to introduce this equipment, whereas, it is, in fact, our aim to introduce something entirely new.⁶⁰

The rest of Brandt's lecture concerned the state of their research on centimetres. He outlined the various projects which they foresaw would be necessary, such as *Naxos*, and other types of early-warning radar. Also mentioned were the various Flak and Naval radars planned for centimetre production, and how work was progressing on countermeasures to centimetre radar. His concluding remarks covered his concern over the lack of personnel at the disposal of the radar industry:

[T]he manpower with which we are called upon to manage at the moment is much too small. We need considerably more men to do all that has to be done. The speed and scope with which this technique will be introduced will be decided by the manpower and not the technical ability to carry it out.⁶¹

This remark indicates how much faith the Germans still had in their technical ability, despite the setbacks that they had suffered in the radar war. They were also aware by this time of the British and American 3cm equipments coming into service, and they suspected that the Allies were probably researching into the 1cm area, which was the case. Brandt's remarks about manpower levels are significant. Taken on their own, we would have to be careful about assigning too much importance to them. They could have been Brandt "fighting his corner" with senior military men, to make sure his project got more support. However, when taken in the context of the supporting evidence from Speer and the archival evidence, we can see that Brandt's complaints were most probably valid.

Over the next year the Germans worked steadily on the new field of centimetre research, experimenting with their rebuilt sets and investigating the field with the resources available. In the Summer of 1944 they conducted trials between their *rebuilt* sets and their replicated ones (*Rotterdam* vs. *Berlin*). The difficulties over fitting an H₂S sized aerial into their own aircraft led them to develop a new type of antenna using dielectric polyrods. This type of antenna was also developed for use in *Naxos*. They also

⁶⁰ AP1136, p631.

⁶¹ AP1136, p635.

commenced work on a *Berlin* Night-Fighter system, which was more suited to the immediate requirements. From October 1944 onwards the RAF/USAF bomber offensive and the increasingly critical situation at the front hampered further research and development, and at the capitulation there were only a few sets of each type actually fitted into aircraft. Examples of these, and some of the personnel who designed them, were captured by the Allies when they entered Germany in May and June 1945. The equipment was examined and the scientists/engineers interrogated. The written summaries of these interrogations, in the form of Intelligence Digests and held in British archives, form the basis of the next section

7.5 Difficulties in Copying, and their Significance

7.5.1 Replication

One of the central themes investigated by this thesis is that of knowledge transfer through technological artefacts. Specifically, it concerns the captured H₂S and AI sets that gave the Germans knowledge of microwave radar. In his 1985 work *Changing Order*, Collins claims that replication of new scientific results is very difficult, if not impossible, to achieve without the transfer of personnel who are able to use their skilfully-acquired tacit knowledge to assist in the process. As he claims:

Proposition Two: Skill-like knowledge travels best (or only) through accomplished practitioners.⁶²

On the face of it, this assertion would appear to have been refuted in the case of microwave radar, Britain, and Germany. The Germans *were* able to replicate the British results; they not only rebuilt H₂S, but designed their own system based on it. If we accept this, what implications does it have for Collins' claims? Or are we able to look further and square what happened with Collins' views.

⁶² Collins (1985), p73.

The important thing to consider is a point I made in the previous section about the difference between general, cultural scientific knowledge, and specific, embodied or embedded practically acquired local knowledge. The centimetre radars that the Germans discovered contained elements of both. The German scientists were highly skilled in the field of electronics, like their allied counterparts. As I discussed, they would be able to deduce what the radars were actually for, and what type of radiation they utilised, by being able to recognise some of the components within the sets that were *not* new to them. This information was supplemented by interrogating the equipment's operators. They were able to work out what most of the unknown components were for by applying their knowledge about what the rest of the components did. What they could not do *initially*, was build working microwave radars. When they first discovered H₂S they did not possess the embodied tacit knowledge about the set that the British had learned by doing it in the first place. However, non-possession of this knowledge did not make replication *impossible*, it just made it difficult as I will show.

When H₂S arrived in Germany it was still a very crude system that required much further refinement. The technology of H₂S was embedded but it still required operation by a skilled person. This was someone who was able operate the controls to produce an image, and who could interpret the image on the screen and use it to navigate with. Interviewing captured operators enabled the Germans to speed up the identification of what H₂S was for. In this case skilled personnel *were* able to provide key information. However the information was not about how to *build* H₂S, but how to operate it. The navigators provided the Germans with the first piece of information they needed to successfully re-build the system - what the "correct result" of operating it should be.

In chapters 2-5 I have chronicled the development of British microwave radar in some detail. It is very clear from that story that the British had a lot of problems developing microwave radar then H₂S. They only solved these problems by a protracted period of trial-and-error experimentation, modifying both their equipment and their ideas about its operation as they went along. I hope that I have been able to demonstrate some of the high level of experimental skill, acquired through several years of actually working with the equipment, that had to be put into the device in order to get it to the operational state. There are not the same types of sources available to tell what happened with the

German investigation of their find, but it is likely that they had to go through the same processes. There are some sources which back up this view.

My account of British development is detailed because I was very lucky to be able to consult original documents in the form of progress reports and some papers that described the development of centimetre component and radar. Furthermore, as Britain was one of the victors of the Second World War, and as radar was part of that victory, many of the developers have written up their experiences as memoirs to show the part they played in developing their “war-winning” invention. This provides another important and rich source of information. I was also able to interview some of the personnel involved who were able to furnish me with other details not mentioned in these books, or give their impressions on how they saw events at certain times. The Germans, most probably because they were the losers, have not written of their experiences in any numbers. What has been written has usually been communicated to other authors in the form of interviews or short, unpublished memoirs. German researchers tended to be older than their British counterparts, and most of them are either dead or untraceable. Furthermore, their archive record is far from complete, for the reason that many records were destroyed or went missing. This makes life more difficult, but there are some original sources which can be used.

So what would have gone through the German investigators’ minds when they first came across H_2S ? This is of course an impossible question to answer without access to any sources, but we know that they were electronics experts and would therefore share the same scientific and technical culture of the British who designed the system.. They would have known that the equipment was electronic, and came from a bomber. Therefore its purpose must have been something that a piece of electronic equipment could conceivably perform in a bomber aircraft, which narrowed down the possibilities considerably. It was mentioned in the minutes of the first *AGR* committee meeting that they recognised quickly that the equipment was a *centimetre radar* of some sort. At first glance this might seem strange, given that in many quarters German theorists did not believe that even if they could produce centimetre waves at high powers (which they could not), they would be any use in radar.

Nevertheless, on closer examination this is not so odd. The Germans had knowledge of radar, and also that one could use radio waves of different wavelengths for many

different applications. They had simply not conceived of the idea of a ground-mapping radar, as they had no use for one. There is no reason to assume that when presented with the physical evidence of how it could be done, by using centimetre wavelengths, that they would continue to believe it was not possible to build one. Their background cultural, scientific and technological knowledge allowed them to recognise what was the likely purpose of the equipment, and on what wavelength it worked, *before* interrogating any prisoners. The Germans had built radar sets, and knew what form they took in terms of the kinds of components and their arrangement within circuits, and the different modules (mixer, transmitter, receiver, etc.) required. It is highly likely that they would have recognised these modules for what they were (a radar), rather than as, say, a radio set. Furthermore, the Germans were also not completely ignorant of centimetre technology as a whole. They did not know of the *cavity* magnetron, but they knew of several other centimetre valves even though these only produced much lower powers. They were also aware of the significance of co-axial cables and waveguides as carriers of centimetre radiation, even if they did not fully understand how to use them. In other words, they did not have the specific skills needed to use these components in radar, because they had not attempted to build one and thus acquire them in the form of practical, embodied knowledge.

In chapter five I described the anxiety of the British over whether to allow the practically indestructible cavity magnetron to be flown over enemy territory. The worry was that the Germans would be able to copy the valve very quickly and easily and use it against the Allies in a relatively short space of time. This anxiety was founded on the British knowing from their own experience of examining captured equipment what the Germans experts, or people with shared cultural knowledge, would be able to do. However, what the British were unable to evaluate was how much general knowledge the Germans had, and how long it would take them to acquire the skills to duplicate the valves and then use them. Despite them knowing that the Germans were unfamiliar with the cavity magnetron, they thought that it would be likely that the Germans would not take long to recognise that it was a high-power centimetre transmitting valve. There were certain facets of its construction that I believe would have been sufficiently culturally general to give its purpose away. Firstly, it was mounted between the poles of a large electromagnet, indicating a high field-strength applied along the axis of the

cathode, just as in the glass-envelope split-anode magnetron. The high field-strength would indicate to them that the valve produced high powers. Secondly, another confirmation that it *produced* high powers was that it had cooling fins to dissipate heat (a by-product of high-power operation) and required a power *input* of several hundred kilowatts. The power supply necessitated large transformers, which were another item of well-understood technology. Therefore, it would be pretty obvious to anyone familiar with electronic components that they were looking at a valve designed to produce high powers at centimetre wavelengths. Furthermore, there were only a few applications that required high power radio waves of short wavelengths, and radar was one of them.

Their shared scientific background with the British would have enabled the Germans to work out the purpose of H₂S. However, just because they knew what their booty was for, doesn't mean they could actually get it to work properly. In order to do this, they would need to acquire much additional information. They would need to know whether they had all necessary components, and replace any that were missing or damaged. Secondly, they would have to learn how to put all the components together to produce the correct result - a PPI picture. The Germans did know, through their questioning of a captured operator, something of what they should expect to see. I will now explore whether replicating the H₂S set proved impossible, as Collins claims, or just difficult. If the latter is the case, then we need to see what made the replication possible, and what form it took.

The Germans themselves appear to have believed that they *were* quite capable not only of replicating centimetre radar, but also of *improving* it. Given the nature of their regime, it is possible that the scientists and engineers involved would make claims that they were as technically competent as the Allies. When their superiors found it hard to tolerate failure, they may well have chosen to argue that any problems with progress were due to deficiencies in manpower and resource allocation (although these certainly did have an influence and cannot be discounted), rather than an inability to do what the Allies had already proven was possible. So failure to replicate by the Germans may have been for one of two reasons. Firstly, their copy may have missed something important because they didn't have a piece of important tacit knowledge, and didn't learn what they had missed at a later stage. Secondly, they may have been impeded in their work because they lacked enough personnel to pursue the investigation quickly and successfully. The

Germans believed the latter, which was true to some extent, but the former was also very much the case.

7.5.2 The German Magnetron

The German copy of the British CV64 magnetron as found in the Mark II (10cm) H₂S units captured, was called LMS10(G). Several examples of the German valve were examined by British experts immediately after the war. Their report makes interesting reading, as it draws out some of the deficiencies and lack of understanding that the Germans had about magnetron manufacture, even though they tried to copy it exactly. The British expert who examined the German copy possessed all the necessary tacit knowledge about cavity magnetron design and manufacture, so he was ideally placed to identify any discrepancies between the two.

The wavelength of LMS10(G) varied between 9cm and 9.14cm (compared to the British CV64's 9.1cm). Its anode block was manufactured from pure copper, which did not aid the process of exact copying as the report stated:

In general the internal finish is not outstanding. The segment holes show machining marks from the boring operation, and the slots in one sample examined by microscope showed a pronounced taper in the radial direction. There is quite a wide variation in the dimensions of the slots in any one block. This is probably a result of the choice of block material.⁶³

The Germans had also not used the Gold-Seal method of the British magnetron (see chapter 3). This is unsurprising, because it was a complicated process invented just before the war by GEC, and would not have been available to the Germans. Therefore they had to use a method that they did understand. They soldered the end plates onto the block, but the results were not as good as they would have been if they had known about and used the British method:

⁶³ AVIA26/860.

The whole assembly is brazed or soldered by an eutectic process, whereas CV64 has gold-sealed end plates. Small particles of solder were found attached to the straps and to the ends of the block, which was in some places pitted by the action of the flux.⁶⁴

It is unclear as to whether the Germans were forced to use pure copper (rather than a slightly hardened copper alloy as the British did), or did so out of ignorance. The small variances in the dimensions of the resonant cavities caused by using a softer material had an effect on the stability of the wavelength of the valve. Wavelength stability was a great problem for the British, too, when they were learning about the valve in 1940-41. It led to them having to test *every* valve they manufactured to measure its precise output. The quality of production was not good because of this, and a significant proportion of the valves were unusable. Microwave circuits were very sensitive to changes in wavelength because the physical dimensions of the components, such as the coaxial cables and waveguides, had a marked effect on the efficiency of the transmitted power. If the wavelength of the magnetron did not match that to which the other components were designed, it would not work efficiently.

Early British magnetrons were also prone to what was known as “mode-jumping”. The frequency of the output would suddenly change to one of the harmonics of the primary wavelength. New valves also had to be tested to see whether they were prone to exhibiting this effect. The problem was solved by the invention of “straps” in late 1941, by Sayers at Birmingham University. They were small pieces of wire linking alternate anode segments, which allowed the magnetron to be pre-tuned to a standardised wavelength and stopped it from changing mode of oscillation.

Tuning the valve by distortion of the strap was done by Allied manufacturers at the factory, thus ensuring that the valve was tuned to a standardised wavelength. However, knowing the significance of the straps, and that they could be used in this way, would not be apparent to a person who did not possess the tacit knowledge of the builders. This was the case when the Germans discovered the valve, and obviously later on because the British did not capture any valves until May 1945. The report related that in this case the Germans simply copied the strap, in mirror image, and appeared to have had no idea of their significance:

⁶⁴ AVIA26/860.

The straps are a mirror image of the straps in CV64 and are set slightly farther away from the ends of the block. It would appear that no attempt has been made to tune the block by distortion of the straps.⁶⁵

This copying without full comprehension is known as “Chinese Copying” in engineering circles. The problem was that by simply trying to copy the artefact, they had no idea whether they had got everything set up properly. So, on the face of it the two may have appeared to be the same, but in practice they hadn’t got the design correct. This was evident, because the Germans failed to achieve the same performance with their valve as they got from the British valves they were trying to copy:

The other three samples [out of eight] gave poor efficiency (approximately 10%) and had a wavelength scatter up to 9.14 cms... It would appear that some form of coding is applied after the initial test to classify samples according to wavelength. The centring of samples near 9.0 cms is compatible with the theory that a Chinese copy of CV64 has been attempted. Before setting of the straps CV64 has a wavelength between 8.9 and 9.0cms and the setting operation consists of increasing the capacity to bring the wavelength to 9.1cms. This may be to some extent confirmed by the presence of some low efficiency samples badly off tune, possibly due to poor jigging and distortion during machining and assembly, no pretuning having been attempted.⁶⁶

The Allied view on the captured German valve is consistent with the idea that they did not have a full understanding of how to make a proper cavity magnetron that could produce the “correct” result every time. They required (i) the possession of knowledge of how to tune a magnetron by strap distortion, which was not obvious from simply copying the strapping, and (ii) certain manufacturing techniques that were either unavailable or unknown to the Germans. This was confirmed in an allied report of the interrogation of Dr Esau of *Telefunken*, who admitted that the significance of strapping was not understood, and that a cast of the straps were made in order to get the “correct” setting. Esau also said that German magnetrons were consistently 5% less efficient (the efficiency was a measurement of power in to power out) than their British counterparts,

⁶⁵ AVIA26/860.

⁶⁶ AVIA26/860.

to the bewilderment of the technicians.⁶⁷ Further confirmation of their lack of complete understanding came from a *Telefunken* report of June 1944. This stated that they believed that the key to tuning magnetrons lay in the relationship between the valve geometry and the applied voltage, rather than in how the anode segments were “strapped” together.⁶⁸ This agrees with TRE’s belief that the Germans had no understanding of how to tune the valve by adjusting the straps. Therefore their own magnetron was not an exact copy of its British counterpart; it was not a *proper* replication and did not behave in the same fashion. However, it did perform well enough for them to use it, so in another sense they *had* managed to replicate the valve. What this tells us is that the transfer of tacit knowledge about magnetron building would have made the replication easier and better, but lack of it did not make it impossible.

7.5.3 Chinese Copies, Collins and Replication

From the evidence of the how the Germans got on with copying the cavity magnetron, we can draw some conclusions about Chinese copying, and Collins’ views on replication. I define a “Chinese Copy” as copy of an artefact that has been done as well as possible, given the level of knowledge of the copiers. The term “Chinese copy” usually means that this has not been done with complete understanding, for the copy is not as good as the original. So what does this tell us about Replication? Firstly, when a replication is being attempted in Collins terms, what is important is that it is the *effect* which is replicated which is the measure of the success of replication.

Collins’ investigation centred on whether a scientist could replicate a TEA-laser.⁶⁹ He knew if he’d done it successfully because the equipment failed to work if he hadn’t. However, in this instance of replication, Collins is concerned with the transfer of algorithmic knowledge. He contends that algorithmic knowledge is not sufficient by itself to allow replication. In this case, the Germans were presented with an artefact which gave them some of the British tacit knowledge - knowledge which had already

⁶⁷ AVIA39/10, Paragraph 206.

⁶⁸ RL39/593, Section X “Bericht des Dr Steimel über den Stand der Zenti-Röhren technik.” [Report by Dr Steimel on the situation of centimetre valve technology]

⁶⁹ Collins (1985), ch 3.

been embedded into the device. Chinese copying *can* allow replication in this instance, because the copier can *unwittingly* transfer the essential knowledge into his copy. But it would appear, from this instance, that having an artefact does not guarantee the ability to replicate equipment successfully if we adhere to a strict definition of success. But it does increase the *chance* of being able to replicate equipment, especially if exact performance copying is not a measure of successful replication. In this case, the Germans had done what they wanted to, but they did not do so with every valve.

That the Germans did not in fact manage to replicate *everything* successfully is confirmed in a report from January 1945. A scientist of the German Radiolocation Commission complained that:

Telefunken A.G. which, inexplicably, was entrusted with the bulk of developments since the last war is unable to copy enemy valves which have been used for years.⁷⁰

The comment shows that the Germans still had problems in copying some valves, due to their lack of tacit knowledge.

The difficulties that the Germans had in some areas with copying, meant that sometimes they didn't copy exactly but used a component or technique with which they *were* familiar. An example of this was the German decision to use a split-anode magnetron as a local oscillator in their *Rotterdam* and *Berlin* sets, rather than a reflex klystron as the British did. This could have been because they actually appreciated the unstable nature of the klystron, as the British found in 1940/1, and were prejudiced against using it. This was the view that Brandt put forward in interrogation.⁷¹ However, there is some evidence that they were initially confused by the fact that the klystron was in a non-standard form, and didn't recognise it as such. Lovell wrote that on meeting Professor Hachenburg in 1977, a fellow radio-astronomer, he learned that Hachenberg had been part of the team assigned to examine *Rotterdam*. According to him, it was not the cavity magnetron which proved impossible to decipher, but the purpose of the "soft-Sutton" (reflex) klystron as part of the T/R set-up.⁷² This view is confirmed by the

⁷⁰ R26III/86, Section 5 "Discussion with Director Speicher of the Radiolocation Commission."

⁷¹ AVIA10/141 Report 14 "Centimetre Valves."

⁷² Lovell (1992), p107.

report of the interrogation of Dr Esau, who admitted to his questioners that they had had trouble with T/R boxes.⁷³ The Local Oscillator klystron employed by the British was not discovered until late 1943, in an intact H₂S.⁷⁴ This was the second set that they discovered. Their confusion over what it actually did, and the late discovery of a conventional klystron, may have been the reason why the Germans used the split-anode magnetron instead. However, some of the German researchers were unhappy with the decision. One revealed in interrogation that he believed that “the British used the klystron for a good reason.”⁷⁵

7.5.4 *Berlin* and the Acquisition of Tacit Knowledge

Despite the problems they had in some areas, on the whole the Germans appeared very optimistic about their research programme. A February 1944 document listing the virtues of *Berlin* compared to H₂S stated that:

The H₂S set had volume of over 21 cubic feet, whilst the German set, which has the same technical performance, has a volume of under nine cubic feet. The weight of the H₂S was 235kg; the German set although made entirely of steel in order to avoid using materials in short supply, weighs only 180kg. When comparing this to other German airborne equipment one must bear in mind that nearly all these are constructed of light metals. Since the same performance was desired, the number of valves could not be reduced; about 50 valves have been retained, but they are practically all normal radio valves.⁷⁶

The claim made by Brandt in this statement was a strong one. It was also not strictly true. The difficulties they had with copying the cavity magnetron showed that this was not the case. Furthermore, the British examined a *Berlin* set immediately after the end of the war, and we can see another view of the Germans' success from what they thought of

⁷³ AVIA39/7 Paragraph 41 “German centimetre research: Dr Esau.”

⁷⁴ AVIA39/10 Paragraph 109 “Development of CRTs, magnetrons, klystrons and triodes.”

⁷⁵ AVIA10/142, Paragraph 6 “Receivers.”

⁷⁶ AP1136, p636.

the Germans' efforts. Their descriptions, summarised by me from the report, are in the following table (italics are mine): ⁷⁷

• TRE Report T1927: Berlin Gerät	
1: Summary	<ul style="list-style-type: none"> • Designed from captured H₂S Mark II. • <i>Circuits very similar; where no German valves existed, copies of British valves were made.</i> • Antenna consists of dielectric elements (not designed for optimum polar diagram). • Scanning speed (6.7 rev/s) faster than H₂S. • Fewer units in installation. • Performance not checked.
2: Introduction	<ul style="list-style-type: none"> • The set examined was probably for naval use. • <i>Berlin</i> was not in general service by the end of the European war. • The aerial is <i>not</i> a copy.
3 Comparison with H₂S Equipment	
3.2 Modulator and Transmitter	<ul style="list-style-type: none"> • The standard H₂S modulator unit is the Type 64. The earlier Type 65 used +2kV and -2kV for the artificial line and spark gap, as does <i>Berlin</i>. (Type 64 uses 0, -4kV) • There is provision for charging choke for line charge. The pulse repetition frequency is 750 as oppose to 1500 for H₂S which implies resonant charging in <i>Berlin</i>. • The output is the same as Type 64, <i>but the pulse shape is not as good.</i> • The integral Modulator Unit power supply is a 450V power-pack (H₂S has a 300V separate unit). • The relay circuit has been redesigned, and is more self contained (H₂S relays are split between the modulator and

⁷⁷ AVIA26/929.

	<p>the control unit).</p> <ul style="list-style-type: none"> • The time delay (Modulator <i>and</i> power unit of H₂S) has been combined into the Modulator. • <i>The spark-gap valve is a close copy of CV85, though smaller and with an improved method of mounting. The general performance of the valve is similar to CV85.</i> • The pulse transformer was sealed, so it was unexamined. • The transmitter valve, LMS10(G) is a copy of CV64.
3.3 Display	<p>Comes in three parts:</p> <ul style="list-style-type: none"> • Power supply, waveform generator, timing circuits and Intermediate Frequency amplifier. • Indicator unit containing height and PPI tubes, local oscillator, output amplifier. • Control unit containing on-off switches, range & height drums. <p><i>In general the display unit is a wire-for-wire copy, except for the use of magnetic deflection instead of electrostatic deflection in the PPI.</i></p>
3.3.2 Waveform Generator	<ul style="list-style-type: none"> • Free-running multivibrator with time-constants of 18 and 60km. The recurrence frequency of 1500Hz is identical to H₂S Mark II. • The output is squared by a further valve, then fed to a Miller integrator valve which <i>gives a linear saw-tooth circuit clearer than H₂S Mark II.</i> • The saw-tooth wave is fed to a phase splitter and the grids of push-pull output pentodes. It has a self contained power pack, rated 300V and 120mA, which feeds this output stage only. • The plate-to-plate output is stepped down by the output transformer and fed to a rotating coil in the indicator. Conversion (voltage to current waveform) relies on pentodes. There is no feedback for linear scan.

	<ul style="list-style-type: none"> • The saw-tooth wave is fed from the Miller valve to a transitron with variable ground bias. <i>This provides a negative synchronised signal for the modulator, which is identical to H₂S Mark II.</i> • The saw-tooth wave then goes to a cathode follower which feeds a high-tension tube. This is a step-up transformer. • The original square-wave phase is reversed by a valve with a small anode load. The signal and markers are mixed in, and the output from the timing circuits is brought in in parallel.
3.3.3 Timing Circuits	<ul style="list-style-type: none"> • <i>These are copied exactly.</i> They retain the anti-flutter stabilising valve. There are changes in the anode resistance from 51 to 47 kΩ, and condensers from 0.01 to 0.05μF.
3.3.4 IF Amplifier	<ul style="list-style-type: none"> • <i>Copied exactly,</i> with a 13 MHz midband frequency and 3 MHz bandwidth.
3.3.5 Indicator Unit	<ul style="list-style-type: none"> • The nearest British equivalent is the “M” screen • There is 4kV between anode and cathode. The flat, 5” tube has electrostatic focus and magnetic deflection. • The deflection coils are driven by a motor.
4 Conclusion	<ul style="list-style-type: none"> • <i>There is very little weight-saving on H₂S Mark II.</i> • <i>Berlin is very similar to H₂S in terms of circuits.</i> • <i>The aerial and its associated units are different.</i> • <i>The plugs and sockets are easy to use.</i> • The set was not operated as a whole. Units were tested individually.

This British analysis noted several things which support the idea that the Germans had still not fully understood all the workings of H₂S, even by 1945. Their inability to achieve the same levels of performance as the British set had not hindered them, though, and in other areas had pursued lines of research to surpass the British equipment. They mentioned that *Berlin* was a wire-for-wire copy in many cases, in terms of the circuits. Where the Germans had identified that a British valve performed the same function as

one they already manufactured, they used their own valve. If they did not manufacture one a valve for a certain task, they copied the British version (as they did with their LMS10(G)). The British were unable to verify German claims about *Berlin's* performance as they had not tested the equipment as a whole. They did test the units individually. Comments such as “pulse-shape not as good” indicate that some areas were deficient, and “improved method of mounting” and “circuit cleaner than H₂S” that they had improved the equipment in others. The British report did not mention the relative sizes of the equipment. By August 1945 the German programme had been running for two years, so it is not surprising that they had managed to improve some components. This supports the idea that they were developing their own areas of tacit knowledge about centimetre radar, even to the point of designing a new aerial.

At the meeting in February 1944 of the *AGR* the Germans' confidence in their own abilities extended beyond their belief in *Berlin's* technical progress. They indicated that they were nearly ready to commence a production run, and that testing would soon begin:

The German set is constructed in such a manner that no fitting or bench assembly is necessary and thus large numbers can be produced without difficulty. We have produced an experimental series of ten sets, five of which have been tested and are ready for use. The prototype, after having had its ground tests, has been installed in an aircraft in the past few days and is now ready for flight tests. Furthermore, an initial series of 100 sets has been planned and production of these will begin in March (1944).⁷⁸

This confidence was unfounded, as by the end of the war only 10 experimental sets had been produced. When they did commence air-testing, they, too, discovered what the British already knew. H₂S, or *Berlin* in this case, required a lot of work to be made to operate successfully. When they began air tests, they found it very difficult to get the performance they wanted straight away. Testing over-ran their schedule and the proposed 100 sets were never built.

At the Rotterdam Committee meeting of the 31st May 1944, some of the difficulties that the scientists were beginning to experience with the equipment were discussed.

⁷⁸ AP1136, p636.

They were running comparative trials between three types of equipment, *Rotterdam Nachbau* (the first, rebuilt H₂S), *Rotterdam Wiesbaden* (a complete, captured example of H₂S), and the prototype *Berlin* apparatus. The problems that the Germans were having were more or less identical to those experienced by their British counterparts during their flight trials. The main problem they faced was that of *Nullstelle*, or gaps and fades in the picture on the screen. At this point they were still trying to identify the relationship between the type of aircraft used and the influence this had on the way the picture behaved. Different types of aircraft had different body shapes, and different places for locating the scanner. These all affected the image produced. This compared directly to the problems the British had in getting the scanner shape right to nullify gaps and fades, (see chapter 5).⁷⁹ They conducted their comparison by taking a photograph of all three PPI pictures over the Müritz See, a very distinctively-shaped lake near Berlin. The pictures are shown overleaf. All three images are indistinct in different ways.

As was noted in the British report, *Berlin* employed a new type of antenna. This was a six-pronged dielectric antenna rather than the paraboloid with waveguide feed that H₂S used. This antenna was developed originally for the German *Naxos* 10cm detector. In a report on this *Naxos* antenna, the British expert who examined it conceded that the Germans had made a significant advance in developing their own solution to the problem of the H₂S antenna being too large for their aircraft (see overleaf for illustrations). he also noted that they had done it by *practical experiment* rather than by theoretical work:

It may also be of interest to record here that investigation has shown that the Germans developed their dielectric aerials first from an experimental standpoint and they subsequently generalised their results on the basis of a satisfactory working theory; they found that the important dimensions for the solid dielectric aerial were the diameters at the beginning and end of the coned portion which determined the aerial impedance and the matching of it to space.⁸⁰

In the next chapter I will assess the contribution made by practical learning (or embodiment of practical skill) in the re-building of H₂S in comparison to Gooding's theories of learning by doing, and Collins' theories of replication.

⁷⁹ RL39/537, Section II.

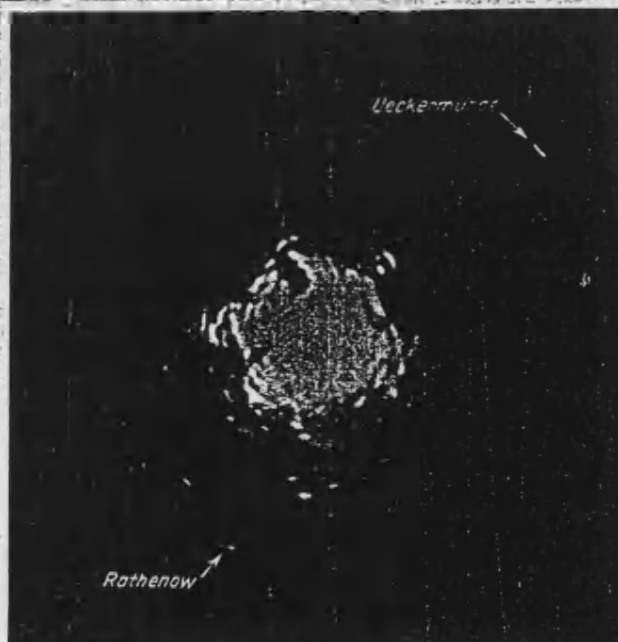
⁸⁰ AVIA26/825.

Geheim!



*Im NO Üeckermünde
im SW Rathenow
als fernstes Ziel.*

Abb.21



Reichweite der Anlage „ROTTERDAM“ Wiesbaden
MÜRITZSEE

DIE ENTFERNUNG ZWISCHEN DEM FERNSTEN ZIEL BETRÄGT 180 KM.

Figure 7.3 Muritz See Map and intact H₂S (Rotterdam-Wiesbaden) picture.

Geheim!

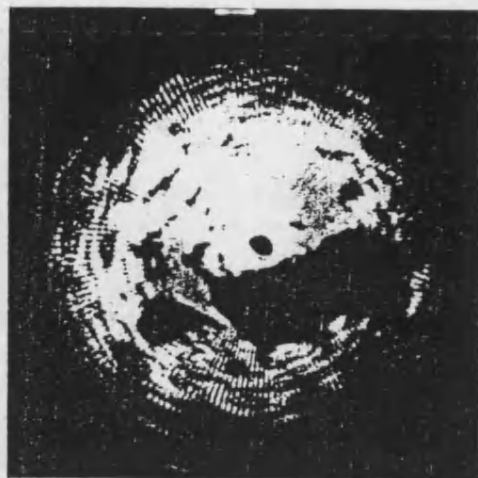


Abb. 20

Aufgenommen mit Gerät, BERLIN*

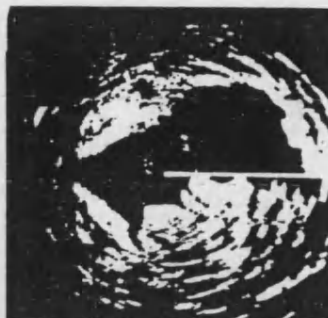


Abb. 19

Aufgenommen mit
Gerät, ROTTERDAM*
Nachbau

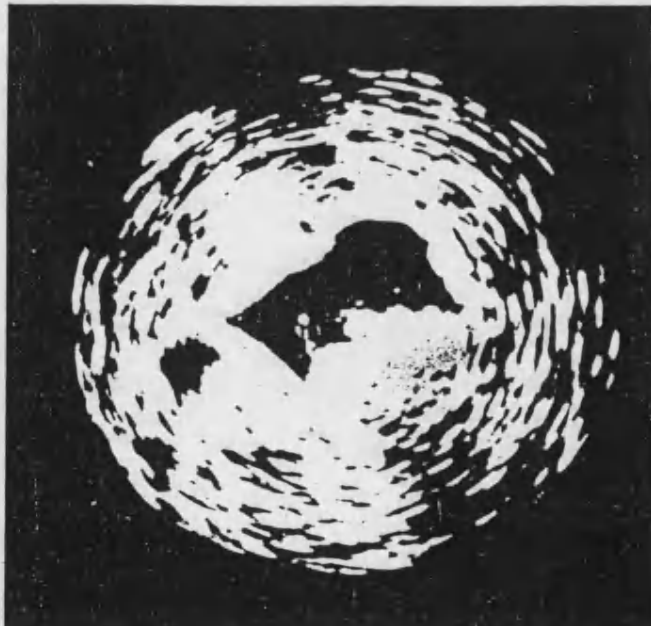


Abb. 18

Aufgenommen mit Gerät, ROTTERDAM* Wiesbaden

VERGLEICH zwischen „ROTTERDAM“-ORIGINAL-Wiesbaden
ROTTERDAM - Nachbau und BERLIN-Anlage

ZA 946 EZ

K. 7/44

Figure 7.4: Rotterdam/Berlin pictures of Muritz See. Comparative photos from rebuilt H_2S (Nachbau), intact H_2S (Wiesbaden) and Berlin. From RL39/537.

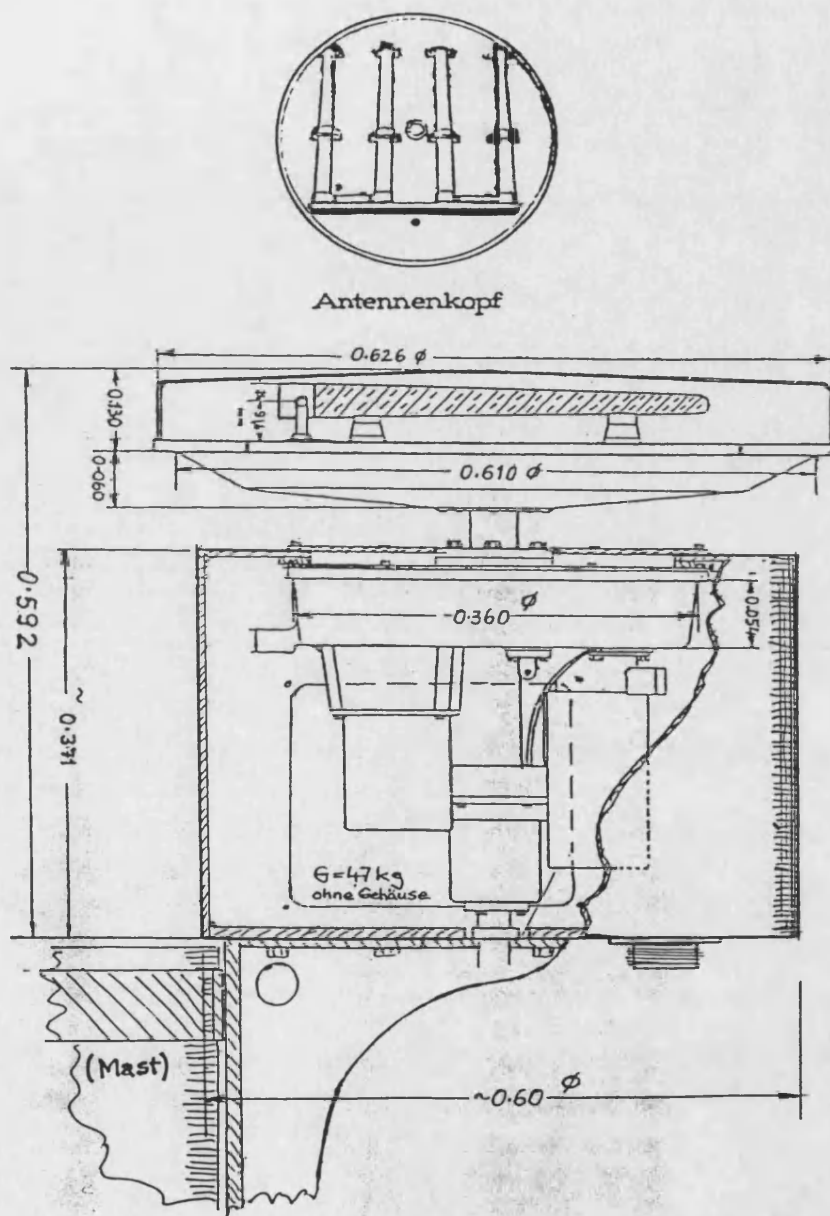
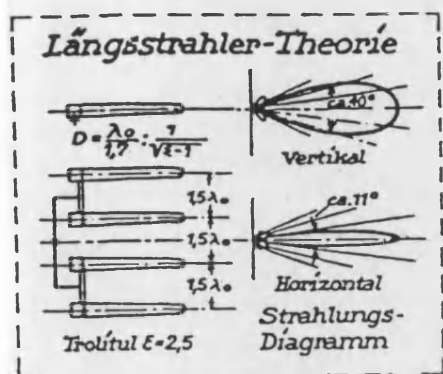


Figure 7.5: Naxos/Berlin polyrod dipole scanner. Specially developed diopole scanner, original to the Germans. it was smaller and lighter than the H₂S scanner, and better suited to German aircraft.

The airborne trials showed the Germans that they had only partially succeeded in replicating the British equipment, in terms of producing an artefact that exactly reproduced the phenomena of the original. This was shown by the comparisons of the performance of their re-built set, and the redesigned set, with the intact original (as the illustrations indicate). The former two apparatus did produce pictures, but they did not match the performance of the original. However, when they set about adapting it and redesigning it to suit their own uses and competences at manufacturing, they started produce images that did approach the British results. This was because they began to learn the necessary skills required to build centimetre radar, as the British did when they first developed H₂S. They were flying with the equipment, modifying it, and picking up embodied practical knowledge about airborne centimetre radar. They also felt that with *Berlin* they had improved on the original in some ways:

The problems of reproducing the British set in the form of the *Berlin* set taxed the combined efforts of our technicians and industry to the utmost in order to make the complicated H₂S set both portable and capable of operating under German requirements. Instead of the 14 component parts, we in Germany have managed with four main parts which, with the exception of one, do not require operating. Instead of the 60 cable leads to connect up the various pieces of equipment, we have 11 multi-plugs. Everything has been done to retain the performance and potentialities of the set whilst adapting it to German requirements.⁸¹

This view was supported in some areas by the British experts, who commented in the report above that they found the German plugs “small and easy to use, as well as other comments that I have already noted.”⁸²

This German report of February 1944 believed, prior to the commencement of air-trials, that the screen-presentation of *Berlin* was as good as the British H₂S. This wasn't the case, as they later found out. Nevertheless despite the atmosphere of optimism about their illusory success, there was a prescient warning on the difficulties of actually *using* the pictures:

⁸¹ AP1136, p636.

⁸² AVIA26/929, Section 4.

It must be mentioned that the performance of [*Berlin*] is dependent on the personnel being able to extract from this ground scanner its full potentialities. We in the industry fear operators will be disappointed when they receive the first sets. They just have to learn to interpret the pictures obtained. The presentation on the British and German sets is certainly the same, the German one may be slightly better. The exploitation of the military possibilities which these pictures provide is exclusively in the hands of the personnel operating the sets.⁸³

By making these points, the writers of the report were agreeing exactly with the British experience again, namely that *possession* of a certain technology didn't necessarily mean that it was useful. In order to use H₂S effectively, the British had to train their operators to interpret the images on the screen. This was a difficult skill to learn, and not everyone managed to do it, as I indicated in chapter 5. In this quote, they also unwittingly pointed out the other major problem facing Germany in February 1944. This was the problem of strategy. The Germans were more in need of effective night-fighters than aids for strategic bombing. They possessed no strategic bombers and were using all their aircraft production capacity to produce fighters.

It is interesting to note that H₂S was proving to be a bit of a double-edged weapon for the British at this time, although they were unaware of it. The *Naxos* receivers allowed German night-fighter pilots to follow the 10cm radiation-emitting bomber stream. Operating H₂S provided the Germans with a powerful indication of the British presence in their skies. Bomber losses mounted at the same time as intelligence reports indicated that the Germans had developed a warning receiver.⁸⁴ On 21st July, Lovell wrote that:

Dalton-Morris, the Chief Signals Officer of Bomber Command visited TRE to complain about the heavy losses over the Ruhr and said that they suspected the German night fighters were getting into the bomber stream by using *Naxos* to home on to the H₂S transmission. Indeed, he said that a captured German night fighter pilot had already claimed to have shot down one of our bombers 'by using *Naxos*'.⁸⁵

⁸³ AP1136, p636.

⁸⁴ This information was gained by listening to transmissions of pilots, which were continually monitored. The information was then assessed by experts, such as R.V.Jones.

⁸⁵ Lovell (1991), p234.

This also shows that the British, too, interrogated prisoners. I am not aware that any coercive techniques were used, but if prisoners gave information freely it had to be checked. There was always the possibility that the prisoner was trying to spread misinformation. Shortly after this, British crews were ordered not to use H₂S, except for a few pathfinder aircraft. This was a big blow to TRE, as it undermined faith in their equipment. Rowe asked Dee to investigate the difficulties.

Earlier in the summer of 1944, Lovell and his colleagues had developed a device called Fishpond as a method of combating night-fighters. The map display produced on the H₂S screen carefully ignored all the echoes received between the aircraft and the ground, in order to ensure that the map image was only of ground features. Lovell had the idea of using the reflections from between the aircraft and the ground on a separate display. This would show all the aircraft in relation to the bomber containing the Fishpond set. It would not show aircraft above the bomber, but the classic interception manoeuvre of night-fighters meant that this didn't matter. Night-fighters would approach the bomber from behind and slightly below. They generally moved quicker than bombers, so this manoeuvre allowed them to pull the nose of their aircraft up and lose speed, matching their speed to that of the bomber. approaching from below also gave the benefit that their aircraft would only be visible from the rear gun-turret, and not the upper turret too, and also would not be silhouetted against the night sky but hidden against the blacked-out ground. The theory of using Fishpond was that aircraft keeping in relative position to the bomber would be other bombers in the stream, and those that moved in relation to the aircraft would most likely be enemy night-fighters manoeuvring into position for an attack.⁸⁶ Fishpond did actually work this way in practice, which showed the remarkable improvements made by the British in the sensitivity of their equipment in the 18 months since the introduction of H₂S.

Nevertheless, something had to be done about H₂S to restore the confidence of the crews. Dee went to discuss its usage with R.V.Jones, and with the bomber squadrons. He wrote a report at the beginning of September comparing both aircraft losses and bombing accuracy with and without using H₂S. He found that bombing losses had decreased from 4% to 2% during the period of H₂S use, and bombing accuracy had increased. He concluded that H₂S was an asset to bomber aircraft. Furthermore Wing

⁸⁶ Lovell (1991), chapter 29.

Commander Saward, the Officer in charge of radar at Bomber Command, discovered that the remarks made by the German POW about *Naxos* were a myth:

Squadron Leader W.H.Thompson of my staff was a fluent German linguist, and I arranged for him to interrogate the German pilot who had recently been shot down and made a prisoner of war and who was alleged to have made statements about the use of *Naxos*. Oddly, the *Naxos* equipment had not been installed in his aircraft, which was reasonably intact in its crashed condition. This interrogation took place at Trent Park, near Cockfosters in Hertfordshire, on 14 October. The prisoner of war explained to Thompson what he knew of the system and how it was used, describing in some detail the method of presentation of the information it received. He also stated, most emphatically, that the device was designed to be used only to locate the bomber stream, the instrument being crude to the extreme, providing no measurement of range or accurate bearing of H₂S transmissions. This German went on to say that for attack the fighter relied on instructions from his Ground Night Fighter Control and the use of his radar interception equipment known as SN2 [Lichtenstein], which was comparable to Britain's AI.⁸⁷

At the same time as Saward was interviewing one POW, information concerning another German device called *Flensburg* was extracted from another. This device was discovered installed in an aircraft which landed in Britain by mistake. It was used for detecting transmissions from 'Monica', a 1.5m active radar which was installed in the rear of bombers to indicate the presence of night fighters. 'Monica' was being superseded by Fishpond, but in the Autumn of 1944 was still fitted to some aircraft. *Flensburg* gave German night-fighter pilots very accurate range and bearing information by homing onto the 'Monica' transmissions. The earlier problems of high losses were attributed to 'Monica', and the equipment was withdrawn immediately. Saward concluded that *Naxos* may well have been of propaganda value to the Germans, in that fear of its usage made Bomber Command cease using H₂S for a period.

During the middle of 1944 thoughts at *Telefunken* and in military circles in Germany turned towards producing a centimetre AI set. After the invasion in June 1944, bomber aircraft which had been attacking invasion targets in France, resumed strategic bombing in Germany. The increased success of the bombers compared to the previous year led to

⁸⁷ Saward (1985), p113.

pressure for a new AI radar. This resulted in the Berlin N1, of which only one was flight-tested before the war ended, in February 1945. The aircraft in which it was installed was captured intact and evaluated, but I have been unable to locate any details of its performance (see overleaf for pictures).

7.6 Conclusions

In this chapter I have related what the situation was like in German research, and how the Germans copied H₂S radar. I have also begun to indicate how the findings of this chapter in relation to the replication of H₂S affect the work of, in particular, Collins. I have developed the ideas of replication in the terms of what knowledge can be transmitted by artefacts. I have looked at what a Chinese Copy does, and whether successful copying requires the transmission of tacit knowledge about how to build equipment.

In the final chapter I want to readdress these ideas in a broader sense, bringing in themes from the introduction and from the rest of the thesis. In particular, I wish to assess what impact my findings have had on the role of scientific practice in creating knowledge, and how such knowledge travels by when it is embodied and embedded. I will also look again at replication, expanding the issues I have raised in this chapter and comparing it to another investigation, on the transmission of knowledge about nuclear weapons. I will then summarise my findings in terms of a reappraisal of some of the literature on scientific practice, knowledge and replication.

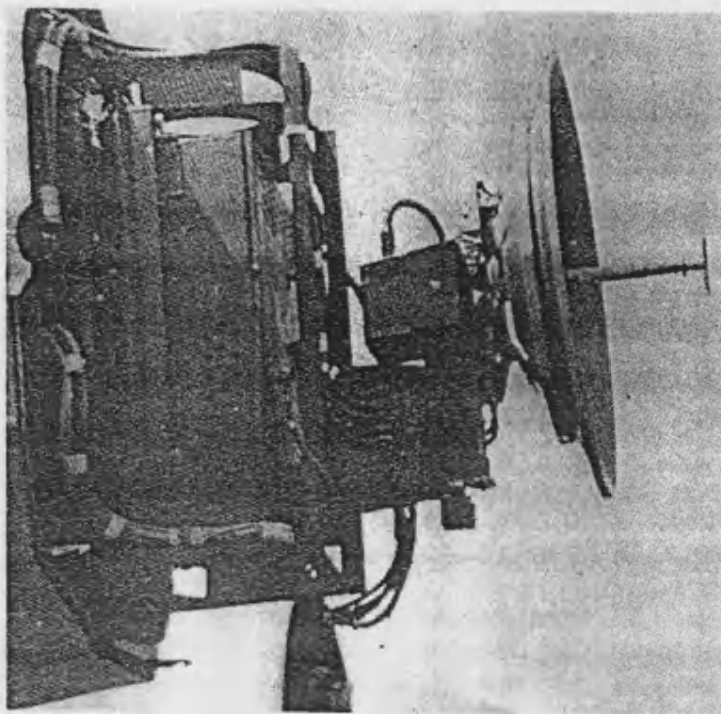


Figure 7.6: *Berlin N1* nose aerial. The scanner for the night-fighter AI version of *Berlin*. From Kummritz (1988), p225.

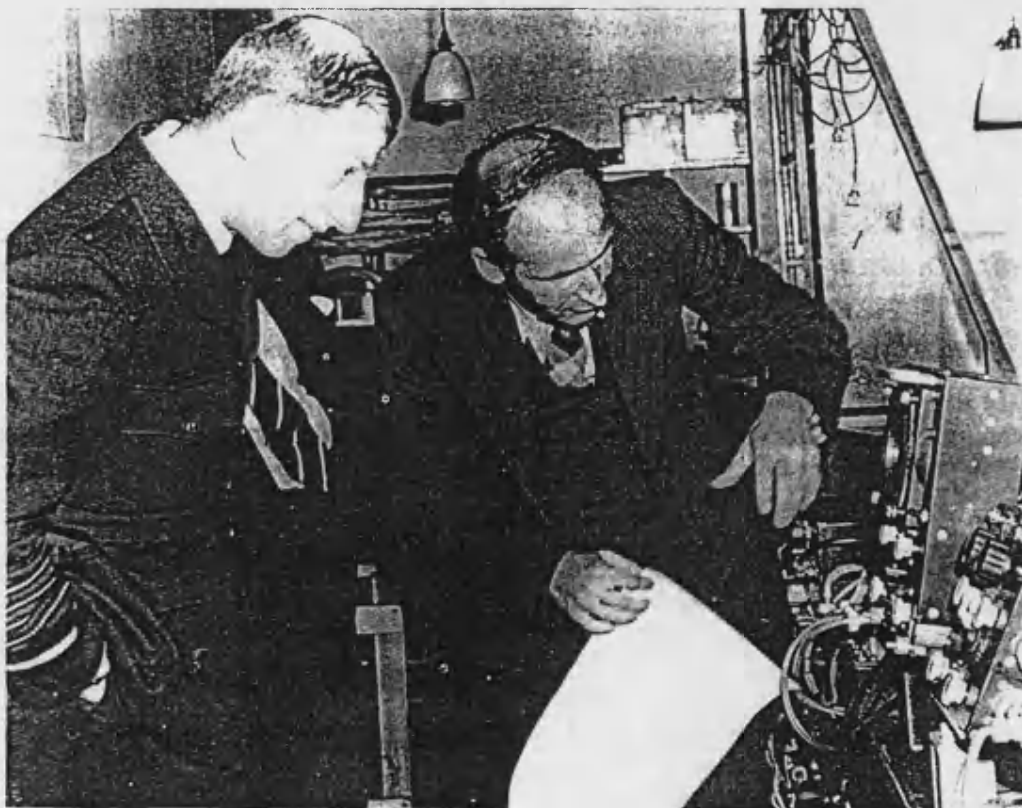


Figure 7.7: Captured German *Berlin* set. Lovell is seen showing the set to Sir Charles Portal, Chief of the Air Staff, in June 1945.

Chapter 8: Conclusions

8.1 Introduction

In this final chapter I wish to draw together the central themes of this thesis, which I outlined in chapter 1. Those themes cover the way scientific knowledge is made, how it becomes embodied as skill or embedded in an artefact, and how it is communicated or transferred. I will assess these ideas in relation to the history of centimetre radar, and to authors within the fields of History of Science and Sociology of Scientific Knowledge whose work forms the basis of the original analysis.

8.2 Practical Experimental Skill and Tacit Knowledge

8.2.1 Background, Common Knowledge

In *The Structure of Scientific Revolutions*,¹ Kuhn set out the idea of the scientific paradigm. The paradigm was the shared knowledge of a community of scientists. In order to become a scientist, the aspirant had to learn a world-view and a way of behaving and doing that permitted him to become fully-fledged. The idea of scientists joining a community, or culture, with unarticulated rules and “ways of going on” is one that has been subsequently developed by other authors in the field of SSK.

Kuhn discussed the idea of groups of scientists sharing the same paradigm, or world-view. He also formed the notion of a serious anomaly within the paradigm triggering a period of revolution, where the old paradigm was questioned, and scientists switched allegiance to a new paradigm that explained the phenomenon which triggered the revolution. He then developed the idea that different fields of science would share certain points of view that bound them to the scientific culture as a whole. However, Biologists, for example, would have different views in some areas to Chemists. Within a field, smaller groups based within national boundaries, or laboratories, or even

¹ Kuhn (1970).

individuals within a laboratory may also not share the same views (or paradigm) on all matters within their field.

The aspects of their beliefs that are shared form the basis of background, common scientific knowledge. These common aspects may take the form of ideas, theories, techniques or artefacts, but it is their shared aspects which allow scientists to communicate with each other. In Kuhn's analysis, failure to reach common ground when an anomaly occurred was the reason for a period of paradigm shift, or scientific controversy. These controversial aspects of science, which highlight the differences in beliefs between communities of science, also highlight the social aspects of science and have therefore been much studied by Sociologists of Scientific Knowledge.²

In chapter 7, I discussed the idea that German scientists shared a common scientific knowledge with their British counterparts. Both British and German radar scientists and engineers had similar scientific backgrounds. They would have had areas of common knowledge about the components and techniques they used, because of their training, and because they had both been developing and building radar for seven to eight years by 1943, when the Germans discovered H₂S. However, they did not share some types of knowledge due to their physical isolation, which was exacerbated by the war. In the next sections I will look at what knowledge the groups didn't originally share, and how this knowledge later did come to be shared.

8.2.2 Embodied Tacit Knowledge

In section 1.2.2 I discussed the origins and subsequent development of the terms "know-how" and "tacit knowledge" within the field of science studies. Whilst many in SSK have looked at science from a social perspective, Gooding and Pickering, amongst others, have examined it from a practical angle. Gooding in particular has looked closely at the fine structure of experiment. By using his own research on Faraday, and Pickering's on Morpurgo, he has developed a schemata of the way scientists acquire knowledge.

² See, for example, Shapin(1979).

Based on Ryle's examination of the difference between theoretical and practical knowledge, which shows that one can *know how* to do certain tasks without necessarily being able to articulate how, Gooding extends this analysis to the experimental arena. Scientists engage the world through the fine structure of their experiments, modifying their ideas, their competences and their apparatus by constant trial and error. The important point is that the scientist learns manipulative techniques that give him a "feel" for how to solve a scientific problem with a certain piece of apparatus.³

When approaching a problem, the scientist will draw on his theoretical, cognitive *and* practical resources in order to decide how to set up his apparatus. The scientist's practical resources can be termed his tacit knowledge. This term, as I explained in section 1.2.2, was first coined by Polanyi⁴, but it has been refined by Collins.⁵ The term refers to the practical skill of the experimenter that is *embodied* within him. Tacit knowledge can be of two sorts, embodied "know-how" that cannot be made explicit, and embodied "know-how" which can. However, with the latter type, as Collins explains, it is sometimes the case that originally one does *not* know that one knows how to do something. In this case tacit knowledge is made explicit when others try to replicate an apparatus, and the replicators cannot reproduce the apparatus and its effect. This causes the original experimenter to look for the reason why, and in so doing, he will make explicit some of the tacit knowledge embodied within him.

I have found Gooding's ideas about the practical acquisition of knowledge through experimental trial and error to be born out by my own work. Radar research and development was almost entirely experimentally based, to the point where the researchers did not have clearly defined theoretical ideas about what they had achieved, even if they did know how to produce an effect. This had a converse, in that there were some experiments where they could *not* get equipment to work properly (as in the case of H₂S), because they had failed to develop the necessary practical skills to make it work.

British radar workers grew up during a period when radio emerged into general society, having previously been the preserve of scientists and the military. The 1920s and 1930s saw a rapid expansion in interest in radio because of the growth in commercial radio stations. A significant number of people built their own radio sets, acquiring

³ Gooding (1990a).

⁴ Polanyi (1958).

⁵ Collins (1985).

practical skills in working with electronics in the process.⁶ Many of these enthusiastic amateurs were youngsters, whose interest led them to enter careers in science and technology in the 1930s, and who subsequently ended up in radar research during the war. Lovell revealed in interview that:

[P]eople like myself, we had been dealing with electronics, the control of cloud chambers and things like that. In my case, I had as a boy been a wireless fan. I was generally acquainted with these things.⁷

I highlighted in chapters 3 and 4 how some of the innovations in centimetre radar came from men who were physicists, rather than specialist radio engineers. The thinking behind using these men was that they would have the experience of being able to work with electronic apparatus, but they would not have the shared background of conventional electronics wisdom which believed that centimetre radar “could not be done”. However, having a physics background was not a pre-requisite, and not all of the researchers in Britain were physicists. Some came from other disciplines. This may seem strange at first, but the situation in University research in the 1930s was much different to how it is at present. Researchers were expected to build a lot of their equipment themselves, with the assistance of the laboratory technicians. This also applied to the non-physicists who joined the radar team, as Lovell described to me:

[W]hen Hodgkin was sent to me at St. Athan, he was a physiologist and I thought he’d know nothing about [electronics] but of course he’d already done the most precise electronic work using cathode ray oscilloscopes on conduction through nerve fibres. It wasn’t unusual in those days for young researchers. The same applied to Pringle who was a zoologist. They were all very clued up.⁸

Practical ability in electronics was widespread amongst British radar researchers. This was less true in Germany in the 1930s, but during the 1920s the same interest in radio pertained.

⁶ Hughes (1993a).

⁷ Lovell, interview with author 1/12/92.

⁸ Lovell, interview with author 1/12/92.

When these men conducted their research in wartime, different pressures pertained to those of their University laboratories. They needed to get their work done quickly, for success was now defined in the terms of a life or death struggle. Their practical abilities stood them in good stead in order to do this. In Britain, Lovell revealed that:

There was not much time for theory at that stage in the war... [O]ne did one's own reading and private cognitions, but there were not theorists about... I think that all the people who assembled at C-Site during the summer of 1940, they were absolutely first class experimentalists, and I think you'd have the greatest difficulty in collecting a similar lot now.⁹

Lovell's views confirm how important practical ability was not only in terms of my analysis of events, which accords with Gooding's views, but also in the eyes of the scientists themselves. They respected the abilities of their colleagues in making the apparatus work. Furthermore, many of the centimetre scientists subsequently had distinguished careers, earning honours such as Nobel Prizes, knighthoods and Fellowships of the Royal Society, so Lovell's comments about the quality of his colleagues are not entirely hubristic.

German researchers, although not as well documented as their British colleagues, had their own struggles between scientists with practical knowledge of how to make radar sets work, and theorists who drew conclusions without recourse to experiment. Centimetre research was cancelled in the first place, because someone with a theoretical viewpoint which discounted the possibility of using centimetre waves in radar had the status to order the work to cease. Later, when that work was reopened, other theoreticians cast doubt on whether centimetre radar would work when it was already being operated.

Embedded tacit knowledge did not only occur in the form of knowing how to *build* apparatus. When the set was manufactured and put into service, people had to be trained how to *operate* the equipment. Very often interpretation of the image on the radar screen was not a straightforward matter, as was the case with H₂S. As I described in chapter 5, learning how to use H₂S was difficult, and not all operators managed it successfully. When they *had* learned how to do it, they'd acquired operating skill, or

⁹ Lovell, interview with author 1/12/92.

tacit knowledge. Given the equipment, a skilled operator would (i) be able to set it up to work properly, (ii) know if it wasn't working properly and (iii) be able to go through simple procedures to try and make it work if it didn't. A skilled operator would be able to reveal what the correct output of the device should be. In other words, they would be able to reveal what constituted the proper "outcome" of the apparatus.

Embodied tacit knowledge is developed in the first instance by individuals or very small groups who work closely together. It is localised knowledge about how to *do* something. However, this localised knowledge must at some point be transferable, or it would remain forever confined to the individual or small group. Gooding has developed the idea further to show how embodied knowledge can become embedded into apparatus. I will discuss what this process entails, and how it relates to radar in the next section.

8.2.3 Embedded Tacit Knowledge

When a scientist or group of scientists commence research into a new area, they bring to bear their intellectual, cognitive, social and practical resources to characterise the problem they wish to investigate. Part of this process involves building apparatus suitable for investigating the problem. This process does not strictly have a beginning and an end: it is cumulative and ongoing. Even when scientists move into a new area, they draw on their reserves of explicit, group knowledge and their tacit, practical knowledge to set up the apparatus they want to use, and to then use it to reproduce experimental results.

However, the world does not always behave predictably, and very often experiments do not work as they are expected. This often occurs in the form of the experiment not working at all. The experimenters then have to modify their equipment to see whether it is possible for them to produce the effect they wish to see, or whether they are going in the wrong direction and a new approach is required. This process of trial and error, and refinement both of apparatus and theoretical ideas, has been described by Gooding in relation to Faraday's¹⁰ rotation motor, and Morpurgo's¹¹ quark detector. Faraday

¹⁰ Gooding (1990a).

modified his manipulations of his apparatus, the apparatus itself, and his ideas and theories about what he was observing and discovering, in order to investigate new magnetic and electric effects. His ongoing discoveries and modifications of his apparatus, technique and theories caused him to change his ideas about what he saw as he went along. This cognitive process also caused him to modify his equipment, as he became better equated with its and his possibilities. The same process occurred with Morpurgo's team, as they strove to design a suitable quark detector, before they even commenced experimentation.

In both cases, the experimenters increased their embodied knowledge about how to manipulate their equipment to produce an effect. When they were able to produce this effect relatively easily, they were then able to modify the equipment so that they did not have to rely on their own skill to produce the effect any more. They could then move onto new areas. In other words, they had embedded their embodied skill and tacit knowledge into the apparatus. The skill now required was the skill of operating the equipment, not the skill of building it.

This process was very important in the history of radar. In order to turn the laboratory apparatus into a production radar, the skill of the original experimenters had to be embedded into the equipment. This was the process of engineering, such that it could be manufactured in large numbers, and operated by persons who were not skilled in building it. The embedding process stabilises the tacit knowledge of the original experimenters into the equipment. Sometimes during the process of learning how to operate equipment, it works better than expected on the first trial. This was the case with the cavity magnetron, and also with the first centimetre AI, as I relate in chapters 3 and 4. Randall and Boot's original cavity magnetron produced far more power than they expected after they got it to work (they had trouble making sure everything was connected up properly to start with). When Hodgkin first flew with his spiral-scan AI, he was surprised to see the ground represented as a straight line on the screen. This unexpected effect was very useful as a horizon indicator.

The first production H₂S sets were not fully stable, as they were prone to break down during operation. Unfortunately, whilst being perfectly capable of operating the set and interpreting the images in order to navigate, the operators could not make them

¹¹ Gooding (1992).

operate if they did break down. They were able to use an algorithmic set of procedures for fault-finding,¹² but beyond that if the set broke down it took a mechanic to fix it. The mechanic would possess more embodied knowledge about the electronics of the equipment, which would increase as he learned whilst doing his repairs, but even he would not be able to build it from scratch. Modification of design flaws had to come from further trial and error experimentation by the original scientists.

The trial and error process would increase the expertise of the scientist in *knowing how* to build radar. At the same time, his expertise would be transferred into the apparatus as embedded knowledge. By so doing, he made the apparatus into an entity that was stable in operation, in that it produced stable, repeatable effects. This stable entity could then be manufactured into production apparatus. Production apparatus could be used by operators who did not have the same level of experimental tacit knowledge as the scientists, and repaired by fitters who, again, did not have the same level of experimental tacit knowledge as the scientists. Importantly, knowledge embedded in apparatus could be *transferred*, which I will discuss in section 8.3.

8.2.4 Communication

A further important process in the acquisition of scientific knowledge, is the communication of that knowledge. In section 1.2.3 I indicated that I would be seeking to highlight instances of the communication of scientific knowledge on three levels. These three levels are within small groups of scientists, between groups of scientists, and between nations.

Collins' work has shown that the transfer of skilled personnel, who have embodied tacit knowledge, greatly aids the *replication* of scientific apparatus¹³ (I will discuss the issue of whether this process is *necessary* for replication, or just *sufficient*, in the next section). When I examined the development of radar, especially the development of the early British metric AI, and then centimetre AI, I discovered that the availability of practically skilled personnel aided the process of development. Within the research team,

¹² RAF Bomber Command "5 Group Aircraft Drills", RAF Museum.

¹³ Collins (1985).

close contact between skilled scientists could facilitate the trial and error process of acquiring embodied tacit knowledge. For example, during the development of centimetre AI during the summer of 1940, the research team occupied a single hut. As a consequence they were exposed to the work of other members of the group on a frequent basis. If a problem with one particular piece of apparatus arose, a colleague, who may or may not have had other practical skills to bear on the problem could be brought in to assist. Frequent discussions and demonstrations within the group led to the sharing of ideas, and colleagues were on hand to teach their skills to others. Clearly, co-operation on this level was a good thing for British radar.

Non-co-operation *within* a group was a much rarer thing. The nearest thing I found to it was Bainbridge-Bell's scepticism about the first early warning experiments in 1935, which I related in chapter 2. He eventually left the group because of these differences. The other instance was the disagreements between Bowen and Rowe which occurred in 1940, and were again related in chapter 2. Strictly speaking Bowen and Rowe weren't within the same group. Rowe was in charge of Bowen, but Bowen was working on a separate field. However, their antagonism meant that the centimetre radar group were removed from the main research establishment where they had access to resources such as skilled colleagues and scientific information. Furthermore, when the centimetre group was re-established at Worth Matravers, Bowen's embodied tacit knowledge was denied to the team.

The second level of communication, between groups, featured very strongly in the development of radar. This type of communication has also drawn a great deal of attention within the field of SSK, for it is when knowledge is communicated between groups, that one can see the social process within science at work. The same dynamics that allow the transmission of embodied tacit knowledge *within* a group, also facilitate the transmission of that knowledge *between* groups. The classic case study of transfer of knowledge between groups is Collins' examination of the replication of the TEA laser.¹⁴ Collins has claimed that the laser was only replicated because of the transfer of tacit knowledge by skilled personnel. In other words, the transfer of embodied tacit knowledge is *necessary* to effect replication of scientific apparatus and effects.

¹⁴ Collins (1985).

The other thing that the interaction between scientific groups allows us to see, is the role of social processes in the settling of scientific disputes. SSK makes strong claims about the role that status has in the outcome of disputes, as another Collins case-study examines. Collins argues that when results are in dispute, the experimenter's regress arises. This occurs when one side fail to replicate, and challenge the results of the first experiment. The first experimenter challenges the apparatus of the second experiment, or the experimenter's techniques. As the dispute cannot be resolved by a claim to nature, it is the status of the disputees which resolves the dispute. The side which enjoys the most support within the wider scientific community will win.

When the free exchange of ideas and information did *not* occur between different groups in Britain, progress suffered as a result. This was clearly illustrated by the problem of minimum range (chapter 2), the dispute between GEC and AMRE (chapter 4), and the American scepticism about H_2S (chapter 5). In all these cases, groups away from the main group (who possessed the practical ability of how to build and use the radar in question) challenged this group about some aspect of their work. In each case, the AMRE/TRE group had the dispute resolved in their favour because they had developed the practical ability with their apparatus to be able to demonstrate their findings to higher authority.

In contrast, co-operation between groups speeded up progress, as was the situation with the experimental cavity magnetron. Megaw's trips to Paris to learn about Gutton's tungsten cathode, and his visits to Birmingham to learn about Randall and Boot's cavity magnetron design, allowed him to come up with a superior engineered cavity magnetron.

In Germany, the political system encouraged a culture of secrecy and competition between the services, industry, and the research establishments. To a certain extent this was remedied with the arrival of Albert Speer, who tried to encourage more co-operation (see chapter 7). He was not able to resolve the situation completely, however, and as the British example showed, fragmentation and non-co-operation had a retarding effect on radar development.

There was of course no overt communication about scientific matters between Britain and Germany during the war. The British communicated their knowledge fully with the Americans, and with other members of the Commonwealth (in particular Canada, Australia and New Zealand), but this did not always lead to complete agreement

as the case of H₂S in Britain and America showed (see chapter 5). In the next section I wish to look at the types of communication between Britain and Germany over the rebuilding and replication of H₂S. There was no personal contact between the builders of the system. What communication there was, came through a transfer of embedded knowledge in the form of the apparatus, H₂S, and embodied skill about using it. Both forms of knowledge are not, according to Collins, sufficient to allow replication. It is this claim which I shall investigate in the next section.

8.3 The Replication of Centimetre Radar

In this section I wish to examine the implications of the successful replication of centimetre radar by the Germans, for the understanding of how replication works. Collins summarises his views on replication thus:

Proposition one: Transfer of skill-like knowledge is capricious.

Proposition two: Skill-like knowledge travels best (or only) through accomplished practitioners.

Proposition three: Experimental ability has the character of a skill that can be acquired and developed with practice. Like a skill, it cannot be fully explicated or absolutely established.

Proposition four: Experimental ability is invisible in its passage and in those who possess it.

Proposition five: Proper working of the apparatus, parts of the apparatus and the experimenter are defined by the ability to take part introducing the proper experimental outcome. Other indicators cannot be found.¹⁵

In these five propositions, Collins concentrates on the social dimension of knowledge transfer. In particular, he is stating that non-explicable experimental skill is a necessary component for reproducing experimental results, and that that skill can only be transferred by personal contact between skilled experimenters.

However, there are two other means of transferring scientific knowledge, apart from via a skilled experimenter. Knowledge can be transferred via a set of algorithmic

¹⁵ Collins (1985), pp 73-4.

instructions, and also via a scientific or technological artefact. The question I wish to settle in this section, is whether scientific knowledge can be transferred by these other means *without* the concomitant transfer of skilled personnel. If we take this strong case to be true, as I believe Collins does (see proposition 2 above), then we have argued that knowledge is *not* transferable without skilled personnel to aid replication. —

In a recent paper,¹⁶ Mackenzie & Spinardi have followed the progress of the proliferation of nuclear weapons from the West to the East. In particular, they have looked at the process of the original replication of the U.S. fission and fusion weapons in Russia and in China. This occurred after the transfer of documentary information through non-skilled personnel only. Mackenzie & Spinardi found that there was no transfer of skilled personnel:

We would not claim that our results are identical with those of the paradigmatic sociological studies of the role of tacit knowledge in science. Thus Collins found that only those who had direct personal contact with a group which had previously had build [sic] a working “TEA laser” were able successfully to build one. That is not the case for nuclear weapons. The Soviets, for example, successfully built an atomic bomb without direct personal contact with the Manhattan Project. The contacts with Fuchs were through the intelligence service intermediaries, and as Collins has emphasised, where tacit knowledge is concerned we would expect that “no-one could act as a middle-man unless he himself was practised in that skill,” which these intermediaries would not have been.¹⁷

So, Mackenzie & Spinardi have shown that it is possible to replicate a highly complex scientific and technological project, solely through the transfer of algorithmic knowledge. What the replicators of the original results *did* have over the original experimenters was the knowledge that the desired outcome was possible. However, despite their knowledge of the outcome, the replicators of the American fission bomb did not have an easy or quick task ahead of them:

[N]either the existence of previous programs, nor the leakage of knowledge from them, have made nuclear weapons design and fabrication a trivial task. None of the

¹⁶ MacKenzie & Spinardi (1994).

¹⁷ MacKenzie & Spinardi (1994), p32.

subsequent programs to build a fission bomb was any faster than the original Manhattan project; several of them (notably the Russian and Chinese efforts) grew to reach a similar scale. Nor was this merely a consequence of technological and industrial backwardness. Even in the 1970s, it took South Africa, by then a relatively advanced industrial country, nine years from the beginning of its uranium enrichment program to being able to fabricate even a single bomb with the simplest of Manhattan project designs.¹⁸

The acquisition of knowledge about nuclear weapons was hardest, in terms of the length of time it took, when the replicating country had no previous knowledge about nuclear weapons. Once that initial hurdle had been crossed, it became easier:

Nuclear weapons technologies are not uniform in the balance of algorithmic and tacit knowledge they demand... The design of a fission bomb is more demanding in terms of the acquisition of tacit knowledge than the design of a “secondary” to turn such a weapon into a hydrogen bomb. Whilst subsequent fission bomb programs were all slower than the American program, hydrogen bombs were faster. It took the Americans over seven years to move from their first fission to their first full thermonuclear explosion; the Soviet Union made the move in four years, and the Chinese in three. The most difficult and most crucial step in acquiring a nuclear arsenal is the first one; thereafter it gets easier.¹⁹

Mackenzie & Spinardi’s conclusion, based on this study, is that personal contact between nuclear weapons scientists was not necessary for the replication of those weapons:

The historical record shows... that direct personal contact with a previous, successful, program is not a prerequisite of successful construction of an atomic bomb... [T]he record of the spread of nuclear weaponry supports only the more modest conclusion; that if only algorithmic and not tacit knowledge from previous programs is available, the subsequent development of nuclear weapons is more like independent re-invention than copying.²⁰

¹⁸ MacKenzie & Spinardi (1994), p32.

¹⁹ MacKenzie & Spinardi (1994), p32.

²⁰ MacKenzie & Spinardi (1994), pp32-3.

MacKenzie & Spinardi state explicitly that tacit knowledge is not *necessary* for replication. What they also state implicitly, is that the replication of nuclear weapons, given only the possession of algorithmic knowledge, required the acquisition of embodied tacit knowledge about how to build nuclear weapons. It was the acquisition of this knowledge that took the time, and gave the programmes the character of “independent re-invention”. This was evidenced by the shorter time it took the Soviet and Chinese weapons engineers to build fusion weapons after the Americans had built theirs. The replicators had by this time acquired nuclear weapon building skill, and, coupled with their knowledge of the outcome (that fusion weapons could be built successfully), this speeded up the later replication.

The case of replication through the transfer of technological artefacts, which I am seeking to show by looking at the replication of H₂S, does not agree with Collins' assertion that the replication requires knowledge transfer via skilled personnel either. However, this case study deals with replication when *artefacts* are transferred, and this differs from when only algorithmic knowledge is transferred.

In the case of H₂S, there were two types of skill that could have been transferred via personnel: tacit knowledge about how to build the equipment, and tacit knowledge about how to operate it. The latter information was transferred to the Germans by an operator, and by so doing he gave them knowledge about what the correct outcome of operating the apparatus should be. However, no radar-building tacit knowledge was transferred. The Germans received a partial apparatus first, in March 1943, and then received a complete one in the Autumn of that year.

As I detailed in chapters 3, 4 and 5, the builders of H₂S and its components acquired a great deal of tacit knowledge about building this radar. They did this by engaging in the sort of learning process detailed by Gooding, and described in section 8.2.2. This process also ensured that their embodied knowledge became embedded in the apparatus. It was through the embedding this knowledge that persons who were not skilled radar builders could acquire tacit knowledge about how to operate H₂S.

The Germans did not originally receive a complete, working H₂S. However, they did receive knowledge about how a complete, fully working H₂S should operate. When they finally acquired a working, complete H₂S they had already begun a programme of research designed for them to begin acquiring tacit knowledge about centimetre radar.

The Germans were able to replicate H₂S, in that they took a working equipment and produced the correct outcome of operating it, or getting a picture on the screen. They had to acquire the more subtle skills of navigational interpretation by using it; in other words by trial and error learning. However, they were also able to replicate it in the sense that they were able to build their own system, that also produced these “correct” results. They only did so by themselves acquiring tacit radar-building skills. Nevertheless, they were able to build their own system in a time quicker than the original builders. This was despite the difficulties inherent in the structure of their research operation (see chapter 7) which acted as a further brake on the speed of their research by preventing close contact and co-operation between researchers. That they did manage to replicate the equipment must have meant that the transfer of artefacts speeded up the process of replication. So we can draw the conclusion that artefacts speed up replication more than just the transfer of algorithmic knowledge. I shall draw further general conclusions about knowledge transfer via artefact, but first I will characterise them by assessing what I believe would have been the case in some situations other than what actually happened.

If the Germans had captured an intact H₂S, but had not captured anyone to tell them what it was or how to operate it, I believe that very soon they would have been able to replicate it in the sense of getting it to produce the correct “outcome” - an image on the screen. This would have been because German electronics experts possessed shared background knowledge with their British counterparts about airborne radar, and *some* centimetre components. Through trial and error experimentation by “fiddling” with the equipment (and possibly reading the labels), they would have worked out how to operate H₂S. Once they knew how to operate it, they would still have had to acquire tacit radar-building knowledge through trial and error experimentation, just as the original builders did. If they had possessed tacit knowledge about how to operate it, as indeed they did (but they didn’t possess a complete equipment), then they could have skipped the process of acquiring operating knowledge by trial and error.

If the Germans had been able to get hold of one of the builders who was sympathetic to them, together with a working example of H₂S, they would have been able to *copy* the set far quicker. This would have been because a transfer of tacit embodied knowledge would have occurred. There is no doubt that transfer of knowledge in this way facilitates

replication. Replication by copying *can* occur without this knowledge, but there is no guarantee that it *will* occur. In some cases, as with the copying of the magnetron, replication was done without full acquisition of the tacit knowledge of the original builder. The original performance, or an acceptable version of it, may be replicated, but there was no guarantee that this would occur. The original artefact contained the embedded tacit knowledge of the builder. This allowed operation by a non-skilled person, but did not make copying a foregone conclusion. The replicator may have been able to copy it without acquiring the same *building* tacit knowledge as the original builder, but unless this knowledge was acquired, then it may not have happened.

In summary, I would propose the following as conclusions to be drawn about replication:

- (i) Scientists share background knowledge through membership of a common scientific culture. The closer scientists are in terms of their subject, their work, and in their physical proximity, the larger this common knowledge is.
- (ii) The process of trial and error experimentation leads scientists to acquire local, embodied tacit knowledge. This becomes embedded into technological artefacts through further trial and error experimentation by the original scientist(s).
- (iii) Persons who are not skilled at building the original artefact, can become skilled at operating it, *because* the original experimenter's tacit knowledge becomes embedded into it.
- (iv) Transfer of algorithmic knowledge gives the slowest transfer of knowledge, because the process of learning by hands-on experimental trial and error, in order to acquire *builder's* tacit knowledge, has to take place.
- (v) Transfer of artefacts, plus shared background knowledge, acts in a similar way to transfer of algorithmic knowledge. Replication of *outcome* can take place once the correct outcome has been learnt through trial and error *operation*. Replication of the equipment in terms of copying *can* take place without the acquisition of further embodied building knowledge, although this is not guaranteed.
- (vi) Transfer of artefacts, plus the process of trial and error experimentation leads to replication by copying. This can sometimes be quicker than the original experimentation, given possession of knowledge of the outcome (how to operate the equipment).

- (vii) Transfer of artefacts, plus transfer of personnel skilled in operation, does away with the necessity for the process of trial and error learning required to operate the equipment.
- (viii) Transfer of artefacts plus transfer of embodied tacit knowledge of how to build the apparatus *ensures* replication takes place, and takes place speedily.
- (ix) Free transfer of artefacts, embodied operating and building knowledge, and algorithmic knowledge is the best way to ensure quick replication.

8.4 Thoughts and Suggestions

There are some aspects of my work that I feel I should bring to the attention to the reader, having completed the writing up of this thesis. Firstly, my research has entailed using one technique not normally associated with the historian, namely that of interview. I also wish to comment on my impressions of my findings, and what I believe to be possibilities for building on my work.

I found the use of interview to be a valuable tool in my research. Naturally, interview can only be considered when there are participants alive to recount their experiences and impressions, which means that its scope is limited to the very recent in historical terms. The most useful aspect of interview for me was that it enabled me to (i) check on unclear aspects of participants' own writings, be they from their original material or from books they had written subsequently, and (ii) I was able to get impressions and anecdotes that are not generally available in primary and secondary sources. With the growth of sound archives and interviews, this resource will be more important to the historian of the future. One proviso is that they will be reliant on the questions and questioners of the past, whereas I was able to dictate my own agenda in this respect.

I was generally extremely happy with the amount of material I was able to find and use on British developments. I was able to make use of a wealth of primary material including reports, laboratory notebooks, diaries and interviews. These enabled me to prepare a detailed record of the genesis of British centimetric radar. For various reasons I was not able to do the same with German radar. I was unable to track down any participants to interview. German researchers, by the nature of their University system,

were older than equivalent British researchers and most if not all of them are now dead (as are many of their British counterparts). The German archives were not as complete as the British for two reasons. Firstly, much German research was undertaken at a commercial company, *Telefunken*. Its modern day successor, AEG, had no record of its wartime work. They were unable to tell me what had happened to their records, except that they suspected they were either removed by the Russians, or destroyed in the bombing or the capitulation, or both. Secondly, other records suffered the same fate as those from *Telefunken*. Fortunately, some material remained for me to use, namely British intelligence assessments and the *Rotterdam* minutes. From these I was able to extract a surprisingly large amount of relevant and useful information.

Finally, I believe that I have been able to make a contribution to the debate about tacit knowledge through my work as I summarised above. I have also created a historical interpretation of British and German centimetric radar development, which has not been attempted before to my knowledge. I believe that there are three further threads that could be continued from my work.

The first would be to compare the work in Britain with the work the Tizard Mission initiated in the USA. In chapter 5 I relate briefly how research at the Radiation Laboratory in the US, which ostensibly was co-operating fully with its British counterpart, did not always come to the same conclusions as AMRE/TRE. Closer examination of this disagreement by me did not fall within the scope or budget of this thesis.

Examination of the Japanese research on centimetre radar is a further area that could be investigated. The Japanese copied some British and American radars, but developed their own series of cavity magnetrons before they saw any Allied versions. I also feel that an investigation of the Japanese research, their copying of Allied equipment, and their apparent non-co-operation with their German Allies would provide fruitful ground for further work.

Finally, an interesting project to arise out of this work would be to follow the process of the building of H₂S in more detail. One would look even more closely at the minutiae of the way the H₂S team operated in the laboratory and conducted their flight trials. This work could then be extended to cover the role of the Operational Research teams, composed of RAF personnel, in the process of development. The personnel from

the commercial firms who would actually build the apparatus would make a good topic for investigation. One could then go on to look at the way the equipment was introduced into service, by following the processes of setting up training programmes for the fitters who would install and maintain the equipment and the navigators who would operate it. Such a study would be able to uncover the different ways in which practical, theoretical and algorithmic knowledge is created, refined and implemented in an unusual situation.

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